NATURAL SCIENCE:

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NOTES AND COMMENTS.

Independence in Scientific Journalism.

MR. ROBERT LOUIS STEVENSON, in his delightful "Letter to a Young Gentleman who proposes to embrace the Career of Art," insists that it is "far more necessary that a man should support his family than that he should attain to—or preserve—distinction in the arts." So also in journalism, it is the first business of a journal to prove the utility of its existence by paying its way. The splendid memoirs of endowed institutions are necessary and honourable, the periodical caprices of the wealthy are amiable follies, but neither are journalism. On either side the path of a scientific journal are many pitfalls. Mere popularity may be lucrative but is a descent from science, mere technical science for the most part requires external financing. But there are pitfalls more perilous because less apparent. Suppose the necessary condition of a journal be attained—that it can pay its way—the next thing is for it to take a strong, fearless, independent line on all questions affecting its subject-matter. It must distribute praise and blame; it must criticise freely where criticism may lead to improvement; it must attempt in every way to serve the best interest of science. For these purposes, especially in matters of blame, the most competent and complete information is necessary.

Now, in England the number of those who are competent to deal efficiently with all matters concerning the advancement of science is strictly limited. Necessarily, most of them are connected with the Universities, great societies, and institutions of this country—connected, in fact, with precisely those social mechanisms the working of which must be passed in review. Natural Science, therefore, believes that in the best interests of science and of individuals a certain part of its columns must be open to anonymous criticism. It is the editorial business to distinguish between anonymity and irresponsibility. In many matters this distinction does not exist: fortunately, in scientific matters the degree of reasonableness of any criticisms is

patent to all whom they concern. We shall continue to welcome all comment that is useful, and to publish it in whatever way seems most useful. We are concerned with science and not with individuals; it is our business to distinguish between merely personal criticism and criticism for the advantage of science. So far as we succeed in this we are confident of securing the approbation of all workers: if ever we fail, or seem to fail, we shall receive gladly advice and correction.

NEW JOURNALS.

THE stream of new periodicals is this month increased in volume by two dealing with Natural Science. The first of these is not strictly a new journal, though practically it is so. *Science Gossip* is reappearing under "entirely new management," and seemingly with somewhat different aims. The present month has also witnessed the birth of *Science Progress*, a "monthly review of current scientific investigation." It is conducted by Mr. H. C. Burdett, of Stock Exchange and hospital fame, and it may be purchased for half-a-crown. Mr. Burdett is assisted by Professor Farmer, who is editor-in-chief, and a number of other gentlemen, who form an editorial committee. He has also secured as "contributors to the earlier numbers" Professors Burdon Sanderson, Dunstan, Ray Lankester, and others. The committee states that "in no case will the articles be of a popular character, although it is intended that they shall be intelligible to persons possessing an ordinary scientific training." The first number contains 104 pages, distributed among seven articles, dealing with "Physical Science and its Connections," "The New Theory of Solutions," "Insular Floras," "Fossil Plants," "Bacterial Poisons," "Vertebrate Morphology," "Chemical Physiology." These appear to be excellent *résumés* of current research, and cover a wide field.

MEMORIAL VOLUMES.

THE multiplication of periodicals not directly containing the results of individual research leads us to the consideration of a type of publication which is both interesting and annoying. Journals, popular or otherwise, which do not affect to contain actually new work, have a notable and valuable place in the hierarchy of scientific literature. But the publication of actual research concerns very practically all those who are engaged on research. The confusion of tongues and the laudable enthusiasm of governments and institutions already render patience a necessary equipment of the Zoologist or Palæontologist. With care, access to many libraries, and perusal of Records, it is nearly possible for him to keep abreast with what appears in the ordinary publications. But there have of late been published a good many volumes, we cannot call them books, of a strangely composite nature, their subjects merely united by the desire to do honour to

some teacher or school, or to glorify some particular professor. As instances we may mention the Macleay Memorial volume (NATURAL SCIENCE, vol. iv., p. 141), the volume just issued by the Linacre Professor of Comparative Anatomy (NATURAL SCIENCE, vol. iv., p. 232), and the Wilder Quarter-Century Book, which latter is a collection of papers dedicated to Professor Burt Green Wilder, at the close of his twenty-fifth year of service in Cornell University, by some of his former students. In some cases the papers contained in the volume have been previously published; in others they appear to be something that the authors wished to get out at the moment and for which they have availed themselves of the excellent opportunity presented under a laudable appearance of hero-worship. But in all cases the connection between the papers is of the slenderest. Now it is not our wish to detract from the honour that should be paid to great men, nor to do anything but praise the papers themselves; but we do submit that this mode of publication is one of the very worst that could possibly be devised, and is by consequence no excellent way of showing admiration. Take the case when the matter is new. The volume is probably too expensive for any but rich libraries or institutions to buy; it therefore does not readily fall in the way of the ordinary student; it is cumbrous and not easily broken up, so that one cannot buy the part that one wants; and, in fact, it combines all the disadvantages of a serial with none of its advantages. It would be more to the advantage of readers, and therefore of writers, that these papers should be issued through some of the ordinary channels of publication, and thus find their way readily into the hands of those who take an interest in the various subjects. When the matter is merely republished, of course, these objections do not apply to so great an extent. But another danger presents itself; namely, that the original pagination is often ignored and even the first place of publication not quoted. Whether this is done from ignorance or vanity the result is to cause much vexation and unnecessary labour to all who have to refer to these volumes, and it is probable that the language used about the person the volume is supposed to honour will not always be of a benedictory nature. Hence we wish to suggest that some other means, more consonant with the needs of workers, should be found for doing honour to those who are thought to require it.

A NEW LEMUR.

TURNING from the consideration of publications to the consideration of their contents, we must call attention to a new form of Lemur, named by Dr. Forsyth Major *Megaladapis*, recently (*Phil. Trans.*, 1894) described by him from a marsh at Amboulisatra in Madagascar. The animal appears from its skull—the only part known—to have been a giant among Lemurs; it had in all probability the bulk of four cats. The size, however, did not indicate anything but a comparatively low position among Lemurs.

It seems that this genus does not bring us anywhere near to the ancestral Lemuroid. Dr. Forsyth Major is of opinion that it was a highly-specialised type, mainly by reason of its tritubercular molars and its small brain. The tritubercular molar has been held by Osborn and by others to be the mark of the primitive Mammalia; but the author of the paper before us thinks differently. In Madagascar this kind of molar occurs in the Viverridæ (Carnivora), Centetidæ (Insectivora), and Lemuridæ. This of itself points to convergence, for the most bigoted adherent of "tituberculism" would hardly insist upon a very close resemblance between these three families. Moreover, each of these three families shows members in which the reduction of the cusps has gone further. The genera in which this occurs show other evidence of degeneration. It is an interesting question whether this ancient and extinct Lemur was contemporaneous with man, as was possible in the case of the Æpyornis found associated with it. The skull examined by Forsyth Major was scratched, as are the bones of the bird, but whether this scratching is due to a sharp-pointed instrument of human manufacture or to the teeth of a carnivorous neighbour appears to be uncertain.

Carboniferous Insects.

We learn from a letter in the *American Journal of Science* (February, 1894), written by Dr. Samuel H. Scudder from Paris, that we may shortly expect Mr. C. Brongniart's work on the fossil insects of the Commentry coal. Glimpses of this extremely rich fauna have from time to time been given to the student, and considerable interest naturally attaches to the complete record. The fact that mining at Commentry is carried on in the open air offers the most favourable opportunity for recognising and preserving the fossils. Dr. Scudder writes: "I have had the opportunity of seeing not only a considerable part of the collection, but also the illustrations prepared by M. Brongniart himself from the choicest specimens; illustrations made with a care and exactitude which leave nothing to be revised. . . . Leaving the cockroaches out of account, to which M. Brongniart will give his attention later, the number of these illustrations, their variety, the extraordinary character of the insects themselves and their rare perfection, leave not the least room for doubt that when his work appears, our knowledge of palæozoic insects will have been increased three- or four-fold at a single stroke, and an entirely new point of departure for the future opened. No former contribution in this field can in any way compare with it, nor even all former contributions taken together. Besides it will offer such a striking series of strange forms as cannot fail to awaken the attention of the least incurious. One may not enter into details, but mention may simply be made of one species, regarded by M. Brongniart as one of the forerunners of the dragon-flies, in which the wings have an expanse of

considerably more than two feet (or about 70 centimetres), and of which several specimens are preserved. It is a veritable giant among insects."

Pedigree Wheat.

A well-known artist, on being asked by an amateur how he mixed his colours, replied that he mixed them with brains. Fortunately for the languishing pursuit of agriculture, there is some prospect of a larger admixture of brains than was necessary when farm produce was protected by duties on importations. The live-stock of England has become famous all over the world because of careful selection by breeders. As Mr. Rendle shows, in his article on the Cross-fertilisation of Food Plants in this issue of Natural Science, as much may be done for food plants as has been done for domesticated animals. Unfortunately, many plants, like fruit trees and potatoes, have been propagated so entirely from cuttings that the necessity of growing from seeds has been lost sight of. But it seems a general law of the organic world that sexual reproduction, securing as it does the advantages of cross-fertilisation and the admixture of strains, is necessary, at least occasionally, to secure the best and hardiest offspring. But in all breeding from seeds careful selection is absolutely necessary. The most expensive tillage, the most lavish manuring, and the blandest of seasons are as naught compared with the selection of seeds from the best plants, and the skilful choice of plants for cross-fertilisation. Animals and plants are born differently, and by careful attention to these natural variations almost anything can be done in a few generations. England is a country well adapted by its range of climate and of natural conditions, and by the high state of cultivation of its soil, for experimental seed-growing, and there is no reason why the pedigree wheats of England should not command the markets of the world just as the pedigree horses of England are sought far and near. The range of our land is not wide enough to yield the enormous crops of Russia, America, and Canada, but we might well provide the best seeds for these larger areas. For this change of industry scientific knowledge is requisite. The councils of many English counties are providing agricultural scholarships to be held at scientific institutions. We hope that the councils will see that their scholars are chosen from among those likely to engage in the practice of agriculture, and that they receive their technical training at institutions where scientific knowledge is combined with enlightened practice. There is still a future for England, but it is a future depending more on the brains of her sons than on the natural fertility of her soil.

An Ammonite Problem Solved.

It is a well-known fact that the various species of Ammonites have a very wide distribution over the earth's surface, and that they

succeed one another in the rocks in the same order though in regions far apart. This fact, though extremely important, is in itself not particularly remarkable, since there are other groups of animals whose species have a similarly wide extension in space and a narrow limit in time. The truly remarkable thing is that the same species often occurs in rocks of a very different character, being found in limestones, clays, or sandstones. This may be expressed by saying that the lithological sequence bears no relation to the faunal sequence; and the apparent conclusion is that the evolution of Ammonites was in no way affected by the environment. Taking their stand on some such facts as these, some have even gone so far as to suppose that the rise and fall of any genetic series was an independent phenomenon, comparable to the life of an individual. This may be, but is it so certain that the facts of Ammonite distribution have any bearing on the question?

This question has recently been discussed by Dr. Johannes Walther in the second volume of his "Einleitung in die Geologie als historische Wissenschaft," a volume which deals with the mode of life of marine animals, especially such as are of geological importance. Without offering any opinion as to the zoological affinities of the Ammonites, Dr. Walther points out that their shells, which are practically the only portion with which the geologist has to deal, are paralleled by the shells of only two genera among recent Cephalopods, namely, *Nautilus* and *Spirula*, which, though differing in every feature of morphological import, yet resemble one another as well as the Ammonites in the possession of a coiled, chambered shell. Now the living *Nautilus* and *Spirula* both live at the bottom of the sea and are very restricted in their distribution; yet their dead shells are found over an enormous area. "In the mangrove swamps of Java, on the sandy shores of Ramesveram, among the volcanic ashes of the Canaries, and on the coral-reefs of the tropical zone; everywhere are the shells of *Spirula* found scattered. No coastal deposit in the whole of the Indo-Pacific province is free from *Nautilus*." In these respects *Nautilus* and *Spirula* differ from all other living Cephalopods, even from the female Argonauta, whose shell, though outwardly similar, is devoid of chambers; and just in these respects do they resemble the extinct Ammonites.

Dr. Walther comes, therefore, to the following conclusions: The distribution of Cephalopod shells provided with air chambers bears no relation to the habits of the living animal. The richness of a deposit in chambered Cephalopod shells does not depend on the distribution or habits of the living animal. The form of a chambered Cephalopod-shell allows us to frame no safe conclusion as to the organisation of the animal. The distribution of chambered Cephalopod-shells bears no relation to the changing character of the containing rock or to the depth of the sea in which it was deposited. Applying these conclusions to the particular case of the Ammonites,

we see that their shells may safely be taken as zonal indices, since they mark truly contemporary divisions of the earth's crust wheresoever they occur; while, since their distribution must have taken place after the death of the animals, they afford us no evidence as to the environing conditions that may or may not have governed their evolution.

Molluscan Mimics.

Our ignorance as to the mode of life of extinct mollusca often lands us in great difficulties, and problems continually occur of which the solution can never be more than guessed at, although, could we have but a glimpse of the once living animals, our questions might receive an obvious and immediate answer. The curious resemblances between species of totally different genera, may be explained in more than one way, perhaps by action of similar environment, perhaps in obedience to dimly-perceived laws of growth; but it is not often that we apply to them the explanation afforded by the biological phenomenon known as "Mimicry," not at least when we are dealing with fossils. A paper recently read by Mr. A. H. Cooke before the Cambridge Philosophical Society seems to show that we might occasionally do worse than venture this answer to the riddle. He reminded us that the shells of *Strombus mauritianus* and *S. luhuanus* differed from those of all other *Strombi* in their close resemblance to the shell of *Conus*, a genus with which they were known to live. He pointed out that *Strombus* was a vegetable-eater, with small weak teeth, while *Conus* was a flesh-eater, with large barbed teeth, provided with a poison bag and duct. *Conus*, then, would naturally be avoided by predatory fish, and any resemblance to so dangerous a genus would be of great advantage to *Strombus*. In the case of extinct mollusca, a similar explanation might occasionally be possible, but, of course, only when the species in question occur in the same locality and bed.

Chaos in Science Teaching in London.

Some time ago (Natural Science, vol. iii., p. 297), in referring a second time to the clever anonymous pamphlet on the Organisation of Science, we urged that the existence of competitive journals and rival societies secured freedom and prevented novel views being suppressed, merely on account of their unorthodoxy. To a certain extent the competition and rivalry among teaching bodies in London secures flexibility and range in science teaching, and the present chaos of institutions, which has come about by natural growth, has its good side. On the other hand, much money and time is wasted by unnecessary multiplication of teaching plant; there is no coördination by which various institutions can as a right utilise each other's advantages. The great libraries, museums, and teaching institutions,

are totally unconnected with each other; only by private courtesy or as members of the general public can those working at one institution gain access to another.

From the point of view of a student the case is still harder. He must throw in his lot with one definite institution, and though his education is at least as expensive, his opportunities, considered all round, compare most unfavourably with the opportunities in the great provincial towns, and at the Universities of Scotland and Ireland. In the case of medical students the disadvantages of present arrangements are peculiarly great. The foremost men in different branches of the profession are scattered among the hospitals, and a student at one must be content with mediocre lectures on many of his subjects, although a few streets off the foremost authorities in Europe may be discoursing on them. Moreover, two students at different institutions pursue nearly the same course, and at the end of their career receive the one, an University degree, the other, a mere qualification.

Report of the Gresham University Commissioners.

We hope that the report of this second Commission will prove a great step towards the removal of some of these anomalies. We do not propose to discuss it in a controversial spirit, but to note its main provisions.

The Commissioners recommend first that there should be one University in London; that its teaching functions should be confined to the metropolis, but that it should continue to be an examining body for students presenting themselves from all parts of the British Empire. To prevent a repetition of the old delays, it is recommended that the necessary modifications of the existing University should be made by legislation, and not by charter, as that would entail the action of the present University itself.

In the matter of examinations for degrees, the Commissioners propose that the certificates of institutions other than the University shall be accepted in lieu of the first examination for degrees, but that the final examinations for degrees shall be the same for internal and external candidates. This they propose, having in mind the necessity of the same standard for the same degree; but as in the reconstituted University the teachers will be a very large body with very great powers, they believe that the present unsatisfactory system, by which teaching is wholly subservient to examinations, will be remedied.

The University is to include six faculties—arts, science, medicine, law, theology, and music. The faculty of science is to include pure science and applied science under different boards of study. The latter is to include engineering, architecture, agriculture, and other subjects of technology.

A University Board is to encourage University extension, and if it be satisfied that any portion of its work is of equal educational value, is to accept it as a portion of the University course.

University College, King's College, the Royal College of Science, the Medical Schools of the Great Hospitals, the London School of Medicine for Women, the City and Guilds of London Institute, and Bedford College are to be among the Schools of the University, and their teachers, when individually approved by the University, are to be members of its faculties.

Great additional facilities for research are to be provided, and these, though not definitely associated with the teaching institutions, are to be open to all teachers of the University.

A senate consisting of a Chancellor and sixty-five other members, of which eighteen will be elected by the faculties, is to be the supreme governing body of the University. Then in order of authority follow an academic council elected by the faculties; the faculties, consisting of teachers appointed by, or recognised by, the Universities; the boards of studies; and convocation.

The Commission finally recommend that a statutory commission be appointed with power (subject to the approval of Parliament) to carry out and give conclusive authority to the recommendations of the report.

We must welcome this report as a solid, fair, and practicable attempt to evolve out of the chaos of London institutions an University worthy of the metropolis.

THE NEW BIOLOGICAL SYLLABUS OF THE CONJOINT BOARD.

THE medical schools of London form one of the most important sets of institutions affected by the Gresham University. For some time, biological teaching at them was in abeyance, save at those large enough to have a fair annual number of students going up for a London degree. Three years ago, the Conjoint Board of the Royal Colleges of Physicians and Surgeons, the Board which gives their qualification to the largest number of medical students in England, wisely instituted an elementary course of Biology, as part of the necessary curriculum. The first schedule was cumbrous and peculiar. It included elementary botany, the differences between plants and animals, a number of types of parasitic worms, the characters of the vertebrates, the difference between Man and the Mammalia generally. The syllabus was neither educational, easy to teach, nor harmonious with the existing courses of biology. A revised syllabus of a much better kind has been issued recently. The theoretical work is associated with a definite series of types, which serve to illustrate the main features of ascending grades of structure. It can be taught along with the course for the London "preliminary scientific," although in certain respects the London syllabus is more advanced.

But a great deal has to be done to prevent waste of energy in the

teaching of science at the London schools. The new university will have, as one of its most difficult tasks, the concentration of teaching in a smaller number of institutions without prejudice to the independence and friendly rivalry of the various hospitals. We suggest that the solution will be found on lines similar to the combined lecture courses in successful operation among groups of colleges at Oxford and Cambridge.

Science Teaching at Secondary Schools—The New Commission.

Those who are accustomed to teach at medical schools, or at colleges and universities, must be struck with the lamentable ignorance and want of method of the ordinary pupils from secondary schools. At some of the larger public schools science teaching is admirably conducted, and those at least who give themselves up to the modern and scientific sides are well prepared for advanced science teaching or for the technical training of the medical schools; but in scientific training the majority of endowed and intermediate schools are no whit better than, as Sir Philip Magnus pointed out in 1876, they were at the time of the Commission of that year. Some students have a scrappy acquaintance with elementary facts, but they are ignorant of the methods of studying nature, and they are totally untrained in observation and incapable of describing on paper the simplest object set before them. In this respect, as in many others, endowed and secondary schools stand in marked contrast to the public elementary schools of this country. Anyone who has the slightest acquaintance with pupils from the two classes must have been impressed by this. Now that the movement in favour of technical instruction has become an actual force, the distinction between the education of the lower classes and the education of the upper and middle classes will become still more greatly in favour of the lower classes. Most of the County Councils of England are now offering higher scholarships for various purposes which will enable a greater number of elementary scholars to reach the higher scientific professions; and, unless great improvements are made rapidly in secondary and endowed schools, not only will the path of the most promising children from the largest section of the community pass by these institutions, but, in the competition of life, pupils of the higher schools will be beaten hopelessly. Boys from elementary schools will pass by scholarships direct to technical institutions, and from these by higher scholarships to the universities, medical schools, and engineering institutes. Going by these routes, they will not be subjected to that tone and leisurely culture which is the boast of endowed schools; but none the less will they succeed in the battle of life, and the total result to the community will be, taking for granted the pretensions of the endowed schools, an absence of culture and refinement from many of the most useful and successful members

of the higher professions. If for these reasons only, we welcome the new Commission on Secondary Education. But although it contains many good names, it is specially weak on the side of the natural sciences. Science is represented on it by Mrs. Bryant, D.Sc., Dr. R. Wormell, Sir Henry Roscoe. We should like to have seen some representatives of the scientific teaching of Agriculture, of Medicine; some representatives of pure sciences like Biology and Geology.

Lord Playfair on University Extension.

Addressing students connected with the London Society for the Extension of University Teaching (on March 10), Lord Playfair paid a wise and eloquent tribute to the activity and utility of University extension. He regarded the hostile evidence given before the Royal Commission on the University of London as having arisen from a misunderstanding of the origin and purposes of extension. Up to the last quarter of the eighteenth century the working classes were separated by an impassable barrier from the learned class, for these spoke in a language the others could not understand. For about two thousand years Latin was the learned tongue, and for about two centuries Greek was raised as a second wall of separation between the learned and the people. University extension was an attempt to extend the higher knowledge to the people who are unable to attend courses during the day. Its main purpose was not to educate the masses, but to permeate them with the desire for intellectual improvement, and to show them methods by which they could attain this desire.

So far as extension limits itself to these great objects it must receive the approval of fair-minded people. But extension covers a multitude of sins, although it is only fair to say that the London Association is less a sinner than either Oxford or Cambridge. In the vast population of London it is possible to get together audiences who will prefer the competent expert to the mere lecturer. But in the provinces it is not so, and the lecturers of the Oxford and Cambridge centres are for the most part successful and popular in the inverse ratio of their special knowledge of their subjects. With this in itself we have no quarrel. An evangelist of the Gospel requires no Hebrew or even Greek. A lecturer whose business is to arouse the people, not to educate them, need be no profound thinker or learned scholar. But, at least on its present lines, the University Extension system is unadapted to the teaching of those who are themselves to teach, and against its pretensions to recognition from Government, or from bodies granting degrees, an emphatic protest must be made. For, unfortunately, on every side we hear it urged by the official representatives of the movement that a great educational work among teachers is being done. Teachers, like others, need stimulation and awakening, but stimulation and awakening are quite other than professional education. The theatrical methods of

the public lecturer are the worst example for the trainers of youth. The London Association has some courses specially adapted for teachers: the Cambridge sequences are useful for teachers occasionally: the Oxford courses are least useful. Courses adapted to the mixed audiences of the provinces have no *direct* educational value, and should carry with them no certificates of any value as part of the professional training of teachers. The best training for these must come from those associated with the actual acquisition of new knowledge rather than with the popularising of the results of new knowledge. The advantage of knowledge to the community is not that it is interesting, but that it is useful: useful in the objective sense, that it is the means by which the forces of nature are bound to the service of man: useful in the subjective sense, that it is the means by which the human mind is deepened and widened. The adult population of the community who have found their places in the ranks of workers have earned a right to be interested and brightened, and the great task of humanising their leisure is the task of the University Extension. But children have to find their useful place in the world, and it is above all things necessary that their teachers should be taught by those who actually have to do with fresh acquisitions and fresh applications of knowledge.

Summer Meetings and the Training of Teachers.

On the other hand, University Extension Associations have arranged, in connection with their summer meetings, courses of the highest value for teachers. In the dog-days the great laboratories stand for the most part idle: no teaching is going on, and the devotees of research are taking holiday. At Oxford, at Cambridge, and at Edinburgh, practical courses for teachers, sometimes in connection with County Councils, sometimes not, have been arranged. These courses, where most successful, have been limited in sphere. An amount of ground that can be covered in four hours' practical work, preceded by an hour's demonstration-lecture each day for three weeks, gives teachers accustomed to mere book-work an insight altogether new into the meaning of scientific work. The afternoons can be occupied by excursions either purely for pleasure or bearing on geology, natural history, and agriculture.

We think that much might be done in this respect in London. We have here many laboratories that stand idle for considerable times during summer; there is no difficulty in getting competent teachers of practical work, and in the elementary schools of London and of the neighbouring districts there are hundreds of masters and mistresses who would rejoice to make use of any opportunities provided them. We commend this suggestion to the London Association and to the Technical Instruction Committee of the London County Council.

Closely connected in purpose with University Extension Associations are two bodies which, so far as we know, did not put forward a claim to be considered in connection with the all-embracing Gresham University.

The P.N.E.U.

A note in our last number (p. 238) has, we regret to find, mystified several of our readers, who thought we were starting an Agony Column, and who have been racking their brains to discover the hidden meaning of P.N.E.U. and A.W.P.L. We hasten to dispel their deplorable ignorance.

The P.N.E.U., we beg to inform them, stands for the Parents' National Educational Union, an association intended to educate parents how to educate their children. Parentship is not, we believe, a necessary qualification for membership; still, many of the members appear to be parents, and are presumably married. Remembering, however, an old saying, we are not surprised to see that many of the lecturers on other people's children have not, apparently, entered either of those blissful states. We ourselves have lately pointed out how much education our educators need (Natural Science, vol. iv., p. 81), and from what we have learned of the work of this association, we are inclined to welcome its endeavours. We understand that there are agencies of the Union in various towns of England, but the leading branch appears to be that of Belgravia, which has for its honorary secretary the Lady Isabel Margesson. This is no impertinent patronising philanthropy that forces its way into the homes of the poor with a tract in one hand and a scented handkerchief in the other; on the contrary, it attacks our great under-educated classes themselves in the gilded ignorance of their own drawing-rooms. Possibly even these parents will resent the intrusion, and ring for the footman to show the P.N.E.U. downstairs; for these parents bring their children up by second-hand. First the nurses (of various denominations), then the governess, then the schoolmaster; these are all, as Professor J. R. Seeley has called them, "professional parents," usurping all the more important functions of both father and mother. They do their best, let us admit; but under this system true education, as opposed to mere instruction, languishes, parents fall into disrepute, sons go to the devil, and daughters revolt. The Parents' Union would remedy this. "It strives," to quote its own leaflet, "to show parents that they cannot, by money payments, divest themselves of their responsibilities. It endeavours to supply them with knowledge and training for their task, to impress on them the absolute necessity for giving careful thought and study to the subject of education, and to show them that without wisdom and knowledge parental love will be maimed, and unable to rise to its true perfection."

The Union uses many means to attain this admirable end. A

few parents in any district take counsel together, and associating themselves with those who can assist in the study of child-nature and training, discuss and invite lectures on various problems and difficulties that present themselves. Libraries are started, training lessons are given, natural history clubs are formed. Many other agencies will arise as time goes on.

The educational advantages to be gained from the study of natural history are recognised by the parents who belong to the Union; who also recognise that children will not, as a rule, become keen about natural history, unless their parents and teachers themselves show an interest in it. But the poor parents were not taught how to study natural science in their youth, and now they do not know how to set about learning or how to be enthusiasts in an unfamiliar subject. Fearful of being found out, the poor papas and dear mammas of the Belgravia branch started a Natural History Club not quite a year ago. This Club is distinctly practical in its working, and those who join are required to do some field-work every summer. An Exhibition of the collections made during the first summer was held some time ago, and was very favourably reported on by Mr. E. Sykes, the Secretary of the Malacological Society. The Club also sold to its members a very suggestive Handbook, drawn up by the Lady Isabel Margesson and Miss Vintner. Encouraged by their success, they propose to issue shortly a more extensive Handbook, to which chapters are contributed by some dozen writers well-known in their own lines. This work will be published by Messrs. George Philip & Son, 32 Fleet Street, and should prove useful to others than the benighted parent.

THE A.W.P.L.

THESE letters stand for the Association of Women Pioneer Lecturers. The objects of this Association beautifully combine egoism and altruism. First, it endeavours to open up a new field of work for women and to organise women. Secondly, this work is the extension of science and culture to places and people of all classes not reached by the University Extension lecturers, and the pioneering the way for those lecturers. A representative of NATURAL SCIENCE, who attended the annual meeting of the Association at University Hall, Gordon Square, on March 7, gathered the following information. The Association was founded by Miss Edith Bradley about a year and a half ago, and as it has not yet succeeded in paying its way, is kept going largely by the exertions of that lady. A woman who wishes to be a Pioneer Lecturer is required to prepare a syllabus of ten lectures and then to deliver one of them as a test lecture. Great attention is paid to proper delivery, and those who do not come up to the standard have to attend elocution classes. The lecturer is also desired to "cultivate a graceful deportment and to appear in a suitable and becoming costume." The staff of lecturers at present numbers twenty-four, of whom eleven are University women that have

obtained honours. The number is increasing, but unfortunately the supply already appears to exceed the demand. If only 300 kind people would subscribe a guinea for three years, they would be allowed to attend *all* the lectures and the Association would be greatly benefited. But would the subscribers?

Our readers will be interested to hear that no less than nine ladies are anxious to pioneer them through the difficult paths of science, teaching them science—moral and otherwise, physiology with or without hygiene; in fact, everything from primitive man to all the duties of housekeeper and mother. Some of these scientific ladies have names not unknown to us; but we confess we should tremble to place ourselves in the hands of one who would lecture us from Greek literature, through the Poetry of the Century (Browning, of course, to be treated as an *extra*), down to the Vertebrated Mammal. Clearly "you pays your money and you takes your choice"; or you don't do either. The science lectures do not seem to have been very popular; at certain lectures given at South Kensington the average attendance was as follows: Geology, six; Animals, six; Botany, ten. The lectures in connection with the P.N.E.U., to which we alluded in our last number, were, however, better attended, and it is intended to follow them up with a course of lectures on Zoology for children.

We wish the A.W.P.L. all success; but we must point out one very serious omission from their lecture list. They give no instruction in the elements of humour. But possibly they think example is better than precept.

Science at the Free Libraries.

A remarkable suggestion which bears upon the instruction of the masses is made by Mr. John T. Carrington in the excellent first number of *Science Gossip*. As a result of a recent tour through the metropolitan libraries, he points out striking deficiencies in them. Illustrated books on popular natural history are to be found in many, but these, for the most part, are out of date and of no educational value. In most cases the income of the library goes in the purchase of fiction, or in general expenses, and the librarian depends on donations for the science section of his catalogue, and must accept whatever comes to hand. He makes the valuable suggestion, that some competent authority should invite the councils of the various learned societies to draw up and revise periodically a list of text-books and authorities dealing with their especial subjects. Although we consider this suggestion most valuable, we do not think it practicable. Members of the councils of learned bodies are not, we fear, likely, as such, to give the necessary advice periodically and satisfactorily. For one thing, most of them are not specially familiar with text-books. Proper advice could be attained more easily by appeal to the teachers of different subjects at the various scientific institutions of London. But Mr. Carrington has made out so excel-

lent a case for the establishment of some kind of guidance, that we hope he will succeed, through the medium of *Science Gossip*, in arousing the necessary attention of authorities.

The Darwin Memorial.

Last month we commented on the praiseworthy efforts of Shrewsbury to do honour to Charles Darwin, perhaps the greatest name associated with that beautiful city. Apparently dogmatic folly is still raising its bray against science. A correspondent writes to us:—

"A prophet has lifted up his voice against the iniquity of the people of Shrewsbury in venturing to propose a memorial to Darwin in his native town. A sign has been sent from heaven, and the spire of St. Mary's Church has been blown down. The righteous then are punished for the sins of the ungodly. The punishment would have been more *ad rem* had it fallen on the Shrewsbury Museum or the mayor's house; but 'the wind bloweth where it listeth.' It is not for wicked people like ourselves to deny the logic of the Reverend N. Poyntz, but he seems to have a marvellous short memory. Are we not right in saying that about a year ago some stones of this same spire fell down, a fact which might have suggested even to the vicar the advisability of initiating some repairs? Really the supposition most charitable to the common sense, if not to the ingenuousness, of Mr. Poyntz is that, knowing himself to be in the wrong, he endeavours to distract attention by casting the blame on those who could have had least to do with this regrettable disaster."

I.

Plateau Man in Kent.

THE further back we push our studies of man, the plainer it becomes that a more comprehensive and at the same time more simple definition of the word "anthropology" must be allowed than is sometimes given. Anthropology is not a science of structure and function simply, but of man as an object of natural history, so that man's ontology and phylogeny must be studied like that of any other animal. Unfortunately, we are so prejudiced by preconceived ideas as to his dropping from the clouds in a highly civilised state, that anything claiming more than a very limited amount of development for him is ill-received. Anthropology has thus been but grudgingly allowed to embrace even Palæolithic man; and all sorts of theories have been invented, in the face of hard facts, to minimise the number of centuries that have passed since Palæolithic times. We have still with us the venerable champion of Palæolithic man—Professor Prestwich, who almost single-handed fought for our progenitors in the valley of the Somme. Since those days, however, this science has made remarkable strides: not only has it made its strongholds impregnable, but it has found it necessary in many parts to extend the dominion of the genus *Homo* away into the geologic past to a degree that was previously undreamed of. Nor has this been done at the expense of the State or by official observers. Month by month, year by year, a body of workers has been plodding along in various parts, gaining here a little and there a little, to which a new importance has occasionally been given by the more detailed work of the Geological Survey. In our own Thames area we have had workers like General Pitt Rivers, Messrs. Spurrell, Worthington Smith, Allen Brown, Greenhill, and Shrubsole, to whose names may be added the less known but none the less deserving one of H. Lewis. Then we have had collectors and philosophers in wider fields, such as Sir John Evans, Sir John Lubbock, and several others. During this time it has been conceded that a division might be made in the science, under the head of Prehistoric Anthropology or Prehistoric Archæology. So long as the study is confined to Neolithic, or even later Palæolithic man, these terms might be allowed to stand; for, so long as our discoveries are confined to river-drifts some hundred feet above present water-levels, it is just possible that no great changes in the surface have

taken place, such as we should require any great amount of geological knowledge to understand. But recent discoveries show that the science does not begin with the archæologist, but with the geologist and the palæontologist: when we come to deposits upon existing watersheds, or others which bear no relation to them, or yet others 400 or 500 feet above these, it is obvious that the subject has entered the domain of Geology. For the principal of these latter discoveries, especially in Kent, we are indebted to the undaunted energy of Benjamin Harrison, a tradesman in the little village of Ightham, between Maidstone and Sevenoaks.

It is now over thirty years since this indefatigable observer began making a collection. From time to time the echoes of the great thought-movements of the day reached this sequestered little village, but they were to Harrison as handbills dropped from a balloon; he was entirely shut out from the scientific world. His struggles and his perseverance; his fighting against want of encouragement and sleepless nights; his early risings and tramps to some spot four or five miles away, so as to be there at sunrise, and to hunt before opening his shop: all these are matters to be read with a relish only when the hero is no more. But, however Harrison's labours might have suffered from want of sympathy, they were soon rewarded by interesting finds. The house of his ancestors happened to stand in the old Valley of the Shode; relics of Palæolithic man found not far from his own shop soon whetted his appetite, and stimulated his researches. He carefully searched almost every inch of the adjoining country in the Holmesdale Valley, adding, as he went along, not only to his finds, but bit by bit to his store of knowledge, until he had managed to pull himself abreast of many of the scientific opinions of the day. Then he chanced to come upon a very beautiful palæolith (Fig. 1), some 70 ft. above the bed of the Shode, which set his mind at work in another direction, and one in which it must be conceded he received but little encouragement for a long time. From the first Harrison had kept a strict account of his finds, numbered them, sketched them into books, marked the position of their occurrence on the 6-inch map of the district, and made notes upon them as they presented themselves to his mind. As he contemplated the skilful work of this weapon, the question suggested itself—"Is it possible that this represents man's first essays at flint working? If so, what a dexterous creature he must have been born; in fact, he must have dropped from the clouds nearly perfect. But is it likely? Is it at all probable that man's evolution began with so highly-worked a flint weapon?" To which his reason answered: "No! If, then, this is not one of the first, where must I look for his earlier attempts, but to the more unwasted conditions, to the gravel patches lying above the present watersheds?" Nor were his labours in these places in vain, for he soon found a number of implements, which, in point of workmanship, were inferior to the majority of those of the valley, and, therefore,—from an evolu-

tionist's point of view—might be considered to be older. There was one thing, however, that hardly fell in with his expectations, and that was, that in the valley there were implements of a type identical with those on the hills. But when he came to compare these two groups, he found that those of the hills were fresh and somewhat sharp, while those of the same type in the valley were, without exception, more or less water-worn. Here, then, was the explanation. The implements of this character found in the valleys were derived from the hills; and this idea also explained the existence in the valleys of several other things, such as erratics foreign to the locality, and types of implements still rougher than those of the Hill-men.

But now the physical features of the country perplexed him. He had evidently got to something older than the oldest existing vestige on the Counterscarp; and the question was, where to look for the earlier home of man, in a period before the genesis of this last physical feature, and when the land-surface was probably 700 feet higher than at present. This surface, said he, must have been continuous with the chalk plateau; and upon the elevated ground of the latter, in its most unwasted parts, we must look to find the earliest traces of man in our district. Then commenced an inch-by-inch survey of the whole plateau for a distance of eight or ten miles; and here his labours were rewarded by considerable finds, at first, perhaps, a little disappointing. He soon discovered that here were to be found some implements exactly similar to the later palæoliths of the Holmesdale valley. His ruder forms were also represented, especially in the deep valleys which cut into the plateau, while the "old brown" deeply-stained flints occasionally found in the valley below were now found in great profusion all over the surface, especially in some places where the land was higher and had been curiously less denuded. Unfortunately, however, not a single section was to be seen upon the whole of the interesting area. Careful searching of the surface, of slight excavations for mangold trenches, of holes for trees and posts, and the deepenings of dew-ponds, and here and there of a well-section, not only showed the existence of the "old brown" flints, quartzites, cherts, and other erratics, but revealed the remarkable fact that the former had been picked up and worked perhaps on one edge, used, sharp edges being abraded in the using, then thrown down again; and further that all this had taken place before the flint entered into the remarkable deposit which so altered the surface of the stone, and changed its colour to that characteristic dark brown.

Here Harrison was sure he had evidence of an earlier form of culture or even of intelligence. The tools were *used* tools rather than *shaped implements*. But from the want of sections and invisibility of beds in juxtaposition it is at present impossible to state positively the exact age of the deposit in which these tools received their colouring. Sometimes they are Eocene pebble flints split by frost,

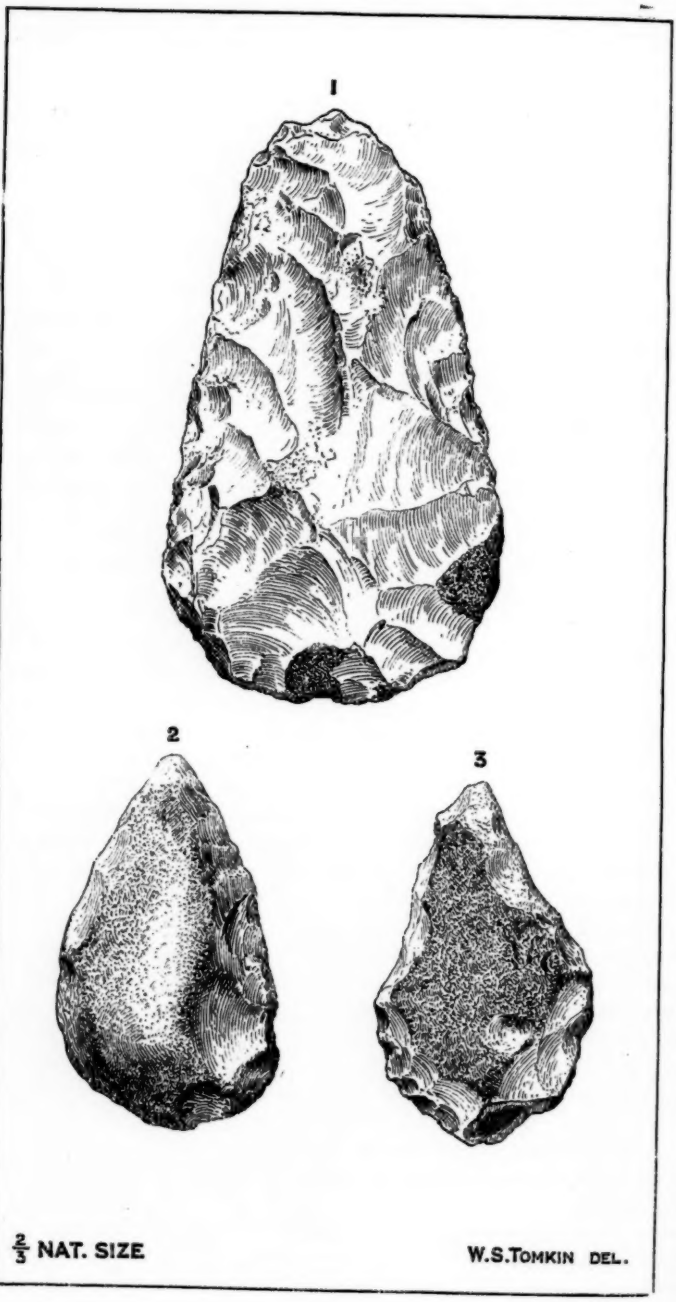
and worked from one side only, at others they are nearly whole flints picked up promiscuously, the working appearing always on the opposite side to a good hand grip. There are no oval hammer-stones such as were used in Palæolithic times, and consequently bulbs of percussion on flakes are very rare. Frequently the flints are striated exactly like some of those in the glacial beds of East Anglia. From these rude used flints numerous groups of implements diverge towards the well-known types of the Palæolithic and Neolithic ages. Just as our palæontologists saw the progenitors of our present ungulates in very dissimilar Eocene forms before the connecting links were discovered, so Harrison saw in his early finds the prototypes of later implements. For years he sought to make his finds known to the scientific world, but his converts were few. However, as time went by, spurred on by his own assurance and the incredulity of others, he increased his collection amazingly. He also distinguished groups of different forms, some of which were most curious. Form, however, was nothing to Plateau man; two objects alone presented themselves to his intelligence, a hand-grip, and a usable edge. Nor was he as yet the victim of one-handedness, left-handed forms being almost or quite as numerous as right. In all his early workings he worked from *one* side of the flint, which he did sometimes with one hand and sometimes with the other, as is evinced by the reversed workings.

While collecting this material Harrison was brought into contact with many well-known scientific men. On one occasion he was introduced to a celebrated archæologist bearing a name classic in geology, who, as Harrison began to speak of Palæolithic man, blurted out in his characteristic way, "Of no interest; prove nothing; in fact prehistoric archæology is played out; my old friend Boucher de Perthes proved all that could be proved." Rather disheartening this, but perhaps better than the action of another set of critics, who, while admitting some of the better finished weapons when shown separately, assumed a position of open hostility to the subject both in season and out of season. Among the various scientists who encouraged him mention might be made of Messrs. Montgomery Bell, F. C. J. Spurrell, and Grant Allen, Dr. A. R. Wallace, and Dr. James Geikie.

In the meantime Harrison had been continually in touch with Professor Prestwich for several years; and as soon as the Professor's leisure would allow he traversed the whole or principal parts of the scene of Harrison's discoveries under his guidance, and from time to time studied these new evidences of a greater antiquity of man. To those who know Professor Prestwich's conservative tendencies and his predisposition to close in the period during which man has been on earth, it will at once be clear how strong must have been the evidence for him to turn round and become the champion of the cause. We well remember the eventful evenings at the Geological Society and the Anthropological Institute on the occasions of the reading of the

Professor's papers on this subject, when the old adage "history repeats itself" was once more verified; for assuredly the Somme finds were never scouted with such contempt as were the poor plateau tools. It is, perhaps, only right to say that we were among the majority. We remember well the impression made upon us by a specimen, which the Professor afterwards figured (*Quart. Journ. Geol. Soc.*, vol. xlvii., pl. viii., fig. 4); but even from this we admit we were obliged to withhold our assent, and we should think none the less of any who did the same in the absence of seeing a series of these things. Fig. 3 shows this to be simply a naturally broken piece of flint with the bark on one side, the right top edge being also a natural break, while any of the flakes struck from the other part are such as might, under exceptional circumstances, be produced in the vicissitudes of gravel making. But, upon closer examination, we notice that the whole of the left edge is worked from the flat side, and that it was formed by some score of blows. Moreover, we see that the resulting edge, till it reaches the butt, lies all in the same plane, and is at a constant angle to the flat side, which implies that every blow must have been administered at the same angle to the surface struck. Anyone who has had a few years' practice in the working of flint would readily admit that this flint was held in the right hand and the blows administered with the left. Further, when we come to the butt, we find that a slight twist exists in the flint, and, accommodating it to the grip of the hand, changes the angle at which the blows were received; consequently the rest of the flakes came off at a lower angle. Now arises the question of mathematical probabilities and the possibilities of "Nature" working in this manner. Could natural forces have administered all these blows on one side, at each time in the right place, when the whole surface of the flint was exposed, and on each occasion at the right angle although there were 180 degrees to choose from? Could natural forces have maintained the blows at a constant angle from the point of the implement till the butt was reached, and then have maintained them at another angle all round the butt? Would all the blows have been struck from the flat side, except when a prominence occurred, and have then been struck from the other side? Finally, would they have attacked the projecting ridge on the top right edge, with the result of turning out a usable implement, however rough? The improbability of such a coincidence of natural blows is so extreme, that though we may regard the shape of this stone as three-parts the work of Nature, we are obliged to ascribe the remaining part to the agency of man.

That Nature suggested this outline to man, or, in other words, that man discovered it nearly or quite at hand, we have not the slightest doubt. In Fig. 2 we have an example with even less work, but when placed with the others, its position in the series is evident. We have but little faith as a rule in illustrations of these rude forms; the specimens must be *seen*, and the physical properties of flint, the laws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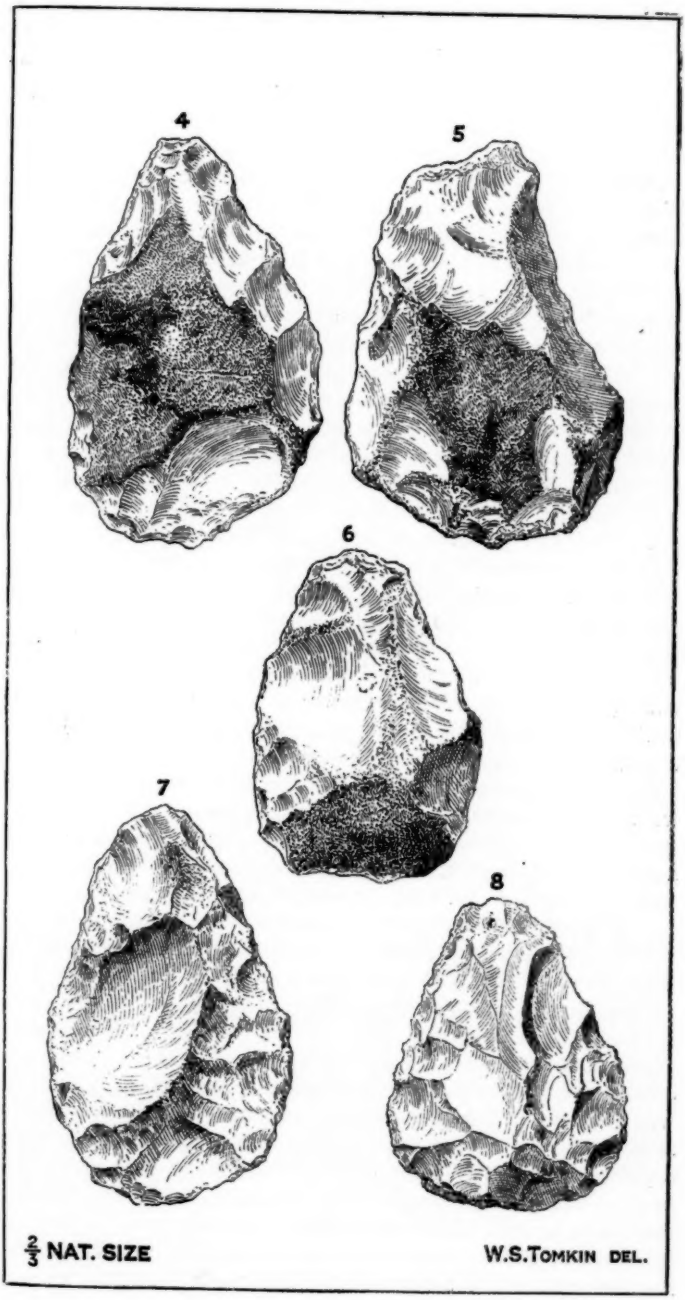
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of force, and the vicissitudes of gravel making, thoroughly understood. In this tool a half pebble is again employed, and from the flat side the whole of the right edge is worked, being struck at a very low angle and made to correspond with the opposite edge. And here again, as in the preceding, the flakes from the point to the commencement of the butt are in the same plane, or at the same angle; and with the change of grip to operate upon the butt, the angle becomes correspondingly changed. A few flakes removed from the left edge, extending about half of its entire length, complete the bilateral symmetry. In Fig. 4 we have got a step further, or, judging by the specimens before us, several steps. Nature, however, still claims the greater part of the work. The large flake from the base is due to the action of frost, but antedates the work on the edge, so that it is practically worked on one side for seven-eighths of the edge, the blows being delivered in different zones and at different angles. On the other side Nature claims a greater share, about one-quarter of the surface being occupied by the natural bark, and about another third by frost-bite; but in this we are introduced to workings on both sides of the edges, which mark a decided advance upon the early workings from one side only. Fig. 5 shows a still further advance; a great deal of the natural bark is still left, but there are decided attempts towards a thickening of the butt, and a tapering from above and below towards the point. Unfortunately, the ancient artisan found he had got too much material to finish with ease and did not quite understand the angle at which flint breaks; but he had the right idea in his head, and probably was far more successful than in this case before he gave up implement making. In Fig. 6 we have a greater advance; the bark is still retained for the base, but the latter is well thickened. Most of the blows, however, were delivered from one side, giving rise to a plano-convexity. This is about the quality of skill achieved by the Hill-men. Fig. 7 marks a still further advance, being chipped all over, and is only one of many specimens which are found in the hill group and occasionally in the valleys.

In selecting from Harrison's collection a half-dozen specimens to illustrate the whole evolution of a type, we are aware of the great gaps that exist between each specimen figured, and of this we are more conscious from the large number of specimens presenting those intermediate characteristics now before us. We cannot expect to prove the evolution, but merely to show the relation of one form to the other. Study of the specimens, however, shows clearly enough that we have here an unbroken sequence of development. In some cases, such as hollow scrapers and bone-splitters, quality of work and condition of material alone separate the Plateau from the Neolithic forms. There are numerous other extensive groups into which the Plateau tools can be divided, in which the archæan character is only surpassed by their constancy of form and recurring numbers. What the uses of some of these tools could have been is as great a mystery

as would have been a fossil boomerang had its use died out with Palæolithic man.

Coming now to the reality of Plateau man, the question, to our mind, is a very simple one. It matters not to us whether many of the cherished treasures of Benjamin Harrison are the work of Nature. We are certain that very many hundreds of them are not. Even if of the 2,625 collected, the odd five can be shown to be the work of man, and their stratigraphical position established, then Plateau man becomes a real being, to whom the modern world was first introduced by the Ightham shopkeeper. At the time of Professor Prestwich's paper the horizon of these implements was not susceptible of exact definition; since then a number of important finds have been made and facts discovered which will go a very long way to settle the question of age. But we dare not enter upon this question now, although we hope to do so shortly, and to prove Plateau man to have been Pliocene.

There is also another great work which Harrison has accomplished at the suggestion of Professor Prestwich since the papers of the latter on this subject. At the date of these papers, the Hill-men were not clearly defined, and implements found at certain heights were associated with them. This group of implements was first noticed by Mr. F. C. J. Spurrell, and he called them the Cave Group ("Palæolithic Implements found in Kent," *Archæologia Cantiana*, 1883). But, so far as we remember, Mr. Allen Brown was the only one to notice that they were of far more modern facies than those of the Hill-men. These have since been proved to have been the work of the Rock-shelter men, and for the purpose of making the necessary excavations the British Association have made grants of money to Mr. Harrison. Anyone who visits this spot will admire the skill displayed in his mode of procedure. Since the occupation of these shelters, the configuration of the country has been so altered that the ground just in front of them, which one would naturally expect to prove fruitful, was not worth working; but, seeing an unwasted ridge about two hundred yards off, Harrison successfully excavated it, obtaining some fifty implements and six hundred flakes. On this ground, and round about the shelters, he had found remains for many years. The implements of this period constitute a distinct group in which the work is of the highest quality ever reached before Neolithic times, indeed the skill displayed in one of the specimens is so great that, when first submitted to Professor Prestwich, despite its shape and altered surface, he was doubtful as to its claim to be called Palæolithic. Many years ago a superb weapon was found in a garden near Sevenoaks Station, beautifully polished all over, which in outline was exactly similar to one of Harrison's finds drawn out half as long again. One of Harrison's specimens, of the Rock-shelter group, was illustrated by Mr. Spurrell (*loc. cit.*). Especial reference may be made to this, as it helps to

bridge the gap between Palæolithic and Neolithic implements. It has usually been considered characteristic of Palæolithic implements that they were used on the point as thrusting tools, and that the broad cutting edge was an essentially Neolithic feature. But in this implement there is a soft spot near the point, and through this a hole has been somewhat irregularly drilled. A point intended for use would hardly have been so weakened; the hole, however, might have served to fasten the flint into a haft of some kind, thus leaving the broad end for a cutting edge, as in Neolithic weapons.

These indefatigable labours of Harrison have thus established the existence in the neighbourhood of five distinct stone periods, although not necessarily separated by a great hiatus, each characterised by special groups of implements. They are as follows:—(*a*) The usual neoliths on the surface; (*b*) The superior late palæoliths of the Rock-shelter or Cave-men; (*c*) The ordinary river drift types of existing valleys and watersheds; (*d*) The Hill group, above existing watersheds; and (*e*) The Plateau group which antedate the present structure of the Weald. Of course, it is only natural to expect to find the implements of all the later periods in various positions in the valleys of the plateau. One thing, however, of extreme importance remains, and that is, that there are large tracts of plateau drift from which all traces of deposits of Palæolithic age have been entirely removed, if indeed they ever existed on them; and in these patches the old Plateau forms are found, to the total exclusion of the Palæolithic types, although the most diligent search has been made for the latter, both by Mr. Harrison and other workers. This disposes of the weightiest argument against the separation of the two races, namely, the idea that the old rude Plateau specimens were the rough work of Palæolithic man dropped upon the surface. It is greatly to be regretted that the necessary funds are not forthcoming to carry out a thorough system of excavations upon the plateau, and to trace the relations of this drift to the undoubted Pliocene beds of the neighbourhood, which we are quite sure would confirm Mr. Harrison's discoveries.

W. J. Lewis Abbott.

II.

Characters in Biology.

IN a recent controversy as to the Factors of Organic Evolution, as well as in other biological writings, many difficulties have been raised, and many phenomena have remained inexplicable, because of fallacious reasoning that occupied itself with terms which have been inaccurately defined, and which mean much less than they are taken to mean. There are several prominent instances of this fallacy, notably in connection with the words *adaptation*, *purposeful* or *zweckmässig*, *variation*, *acquired*, and others; but by far the most remarkable case of the uncritical use of an ill-defined term may be observed in most of the speculative writings where the word *character* is prominent, and the same is true of the German *Eigenschaft*, or *Merkmal*. The frequent term *hereditary tendency*, used as it is in a sense not greatly differing from the word *character*, is also a great source of confusion, and needs the same examination as does the latter.

"*Character*" appears on every other page of our biological works, and is a term we could not well do without. It has always had so obvious a meaning, and is now from long use so familiar to the ear, that it is difficult always to keep in mind what the limitations of the term should be. This character is, or is not, inherited; that other tends to vary: these are plain and intelligible phrases, and are apparently incapable of introducing serious error when used in the study of organisms. But as a matter of fact the term is extremely wide; it may, indeed, include almost anything, yet it is often used as if it had a definite meaning, or answered to real distinctions of parts or relations of the organism, or as if a *character* could justly be regarded as a unit in the study of heredity. The characters of an organism may be new or atavistic or neither; acquired or congenital; individual, specific, or generic; quantitative or qualitative; of a part, of an organ, of a function, or of a relation; or even the negation of any of these; and, yet, utterly incoördinate as they are in kind and amount, utterly arbitrary and changing as are their limitations, they are constantly regarded as units, not only rightly, to the observer, but wrongly, to the organism; are talked of as being present or absent, or as varying in one way or another without affecting the rest of the body, and as only occasionally being connected with one another so as to show a relation of interdependence. Even where

the idea is not so definitely formulated as in de Vries' chapter on "the mutual independence of the hereditary characters," we are constantly met with the assumption that the aspects of the organism, or of its parts, looked at from a hundred different points of view, and selected for consideration in a rich variety of ways, have a special significance as units for the study of inheritance, being treated of as independent of one another in heredity and variation, and often, for that reason chiefly, being regarded as represented in the germ by independent and special material vehicles. A dog, for instance, according to the view of that writer, inherits some of his characters from his mother, some from his father. Some come from a remote ancestor, and others are quite new. One of the parents may have had more to do with him than the other, for in the list of characters it may be that there are ten from the father and only seven from the mother. In most cases the characters are not the mean between those of his parents, they do not even vary all in one direction. There is no obvious interdependence between them. They are, therefore, according to de Vries, to be referred to different material foundations in the germ, derived from various ancestors, which *Anlagen* may vary independently of one another, each working out the character it represents. There are material vehicles for each of his characters, as, among others, for the length of his legs, for his swiftness, for his bark, for his love of sugar and his fear of snakes, as well as for the absence of all that he is not. All these are hereditary characters, varying to all outward seeming independently of one another, and all due to the ultimate *Anlagen*. Now, while few go so far in isolating characters as de Vries does, yet their independence of one another and their unity in themselves is so generally accepted that it is worth while studying what is the principle on which phenomena are grouped together and called a character, thereafter to be treated of as unit, and on what principle they are grouped to form two or more characters, varying independently or in correlation with one another.

The term may, in the first place, be used generally, of the whole creature as individual, with or without any special reference as of size, colour, habit, etc.; and this is generally Weismann's use of the term. Here there is no artificial division of the organism into parts that have no answering physiological divisions. In agreement with this point of view, Weismann's determining foundations in the germ answer each of them to the complete character of one individual ancestor, which is impressed on the whole of the new organism by the little piece of matter in charge of that character and by the little pieces derived from it, whereas those of de Vries answer each of them to one artificially limited character of many ancestors, which aspect of the new organism they look after, neglecting the rest of the body. As Weismann often, however, uses the term character in its narrower sense, he is led into the common fallacies at times. Indeed, an

animal is generally thought of and observed in some special direction, with the result that some appearances soon eclipse the others and come to be regarded as the characters *par excellence* of the form. Anything particularly striking to the eye or fancy at once assumes undue importance and becomes marked off as a unit. And still more often, those characters which are of the highest definitive significance, and consequently, as a rule, of the lowest physiological importance, those which are most "characteristic," take a more prominent position in our minds than others, quite rightly for some purposes, but wrongly for the study of inheritance. For thus one aspect of the organism comes to stand for all.

But, as a rule, characters are quite freely regarded as separate from one another and as independent of the rest of the organism, and are formed arbitrarily and used singly in the observation of the facts of inheritance. The animal is made up of, or at least capable of definite analysis into, separate incoördinate individual appearances, the presence or absence of which, in various proportion, determines its specific and individual relations. And in marking off characters from one another, one principle is always present, though loosely adhered to; that is—that those appearances which usually vary without any obvious accompanying change in the other relations of the organism, are included in one term. Thus a patch of colour is one character, being present or absent from animals otherwise not different. Any relation of paired parts is always called one character, not two, because such always bear a fixed relation to one another. An instinct or habit is a character, being for us irreducible to its complex causes. But any phenomenon for which we can find a cause, or even a distant causal relation in another, is grouped with the latter as one character. Wherever our knowledge of physiological relations between parts breaks down, there we too readily assume a breach in organic relations, calling all that lies between such breaches a unit that varies independently. So, to modern biology, a seeming independence in quantity or quality of one or many structures or functions or both, from the rest of the organism, together with a real and obvious causal relation between the former, are enough to give them the status of a unit for heredity, having a set of little determining demon particles all to themselves. And yet we are asked how it is that characters can vary independently of one another; and ingenious theories are built to account for so strange a fact and to fit an apparent union on to this disunion. Why, but for the very simple reason that just those phenomena which, to superficial observation, range themselves in independent groups are chosen for the dignity of being styled separate characters. Why A = B, is a question that needs little discussion if higher up the page you have defined A solely on the ground of that equation. If this be not the principle of definition of a character, why consider the length of right and left legs not two relations but one? If they ever differed seriously in

length they would be distinguished as two. Now, if this is really the principle on which characters are to be defined, it is superfluous to ask whence the independence in variations. That independence rests on ignorance of causes, and is restricted, step by step, with the growth of physiology. Were all the relations of function of the parts of an organism known, it is simply inconceivable that the quantity or quality of any part you like could be regarded as without effect on the rest, or as having come to the condition in question without the co-operation of the rest. And even if such a hiatus in the relations of parts were thinkable, we have ample evidence that it does not occur, in the cases of obscure correlation of phenomena of various kinds in various parts apparently unconnected with one another in any special manner. In every living thing the islands of connected changes which rise here and there above our sea of ignorance are united on a common bed, perhaps not so deep down as is usually taken for granted.

This false isolation of characters in the adult, and with regard to inheritance and variation, is the chief cause of the assumption of demon particles; and in this connection it may be said that in the expression *latent characters*, which are carried through one generation or sex to appear in another, whether in cases of dimorphism, polymorphism, or atavism, we have a metaphor that often causes error, though it would be pedantic to object to the convenient phrase. The character simply does not exist in the intermediate generation, and no carriers need be invented for it. To say that an appearance is latent is to say that it is not. Though there were waves yesterday and will be waves to-morrow, we do not think of saying when the sea is calm that they are now latent in the sea; we simply say that there are no waves, though we know that when the conditions arise they will appear. A character is simply the appearance caused by the reactions of circumstance and the mass of cells derived from the germ, and in the absence of certain conditions certain characters do not appear. This has been well shown in respect to one class of characters by the valuable essay of Geddes and Thomson. The action of circumstance must come into our account of the rise of alternative structures and functions somewhere; it is surely more reasonable to regard it as directly answerable for the changes in question, and not simply indirectly as awakening the demon carriers of one set of structures which thereafter supersede the demons of the alternative set which now become latent. For thus we have the support of experiment, and substitute a physiological account for what is at best only a bad formal one. One might as well say that men have within them the carriers or determinants of a latent black eye, which are called into action by a blow, as that the difference in form and function of the sexes is due to the action of alternative sets of "determinant vehicles of hereditary characters" one of which triumphs over the other. If one alternative form differs from the other in one respect,

it differs in all; it is against all physiological and pathological experience to think of, say, the sexual characters as independent structures and functions superimposed on a common neutral basis.

If characters are not regarded as varying independently of one another, or as being present in some mystical way in the forms which "transmit" them without "showing" them; if they are not looked at as being any definite unities for the organism, but are recognised as arbitrarily-defined groups of phenomena; if, in short, we make it clear to ourselves that they are only appearances of the individual, which cannot with justice be divided into parts in the consideration of heredity, and which if it varies in one respect varies in its whole constitution, then the result for our theories of heredity is obvious. The questions become at once more complex and more simple. More complex because, being deprived of our false units, we have to leave tabulating characters, and have to go to school to physiology that we may trace the many aspects of the individual to the functional peculiarity of the germ, the many differences to the one, just as the above-mentioned authors have done for the complex phenomena of sex; and because a character ceases for us to be transmitted or carried latent as such by the demon particles, and has instead to be referred in its rise to the reactions of every part with every other and with the world. They become more simple, because we shall cease to be hampered by the difference in the habits of authors as to the way in which they form characters, or by the difficulty of reducing positive and negative relations of quantity and quality, utterly incoördinate and incomparable as they are, to a common formal expression; and most of all because we shall cease to be confronted with the question as to how the demon particles do their strange work.

Surely it is an unprecedented thing that men going about to explain the shapes of animals and plants should leave on one side what are clearly the causes of these shapes, namely, the processes of development which lie to their hand in physiology and embryology, and should rest content with an antique and fantastic scheme of growth which makes all intermediate processes of no value; so that all the changes known as inheritance and variation can be studied in relation to only two moments in the whole of life, namely, the adult, as pure *morphe*, and the germ, fixed and stained. And since this has been to so great an extent our method, it is not surprising that Natural Selection is loaded with more than it can bear, or that the expression which the great principle of use-inheritance receives is artificial and exaggerated.

GEORGE SANDEMAN.

III.

The Cross-Fertilisation of Food-Plants.

SUCH a Society as the Royal Agricultural, which boasts the motto, "Practice with Science," could not better carry out its principles than by encouraging experiments in the cross-fertilisation of food-plants. Darwin, we know, put forward conclusive evidence in favour of cross-fertilisation as a general rule for the production of the strongest and most fertile offspring, and insisted on the advantage or necessity in all cases of an occasional cross. These are points which scientists universally accept; yet so great is the gulf still between practice and science, that the vegetative method is to-day often the only one used in the propagation of our most necessary crops; *i.e.*, this year's plants are produced, not from the seed, but from cuttings or some detached portion of last year's plants.

The potato plant, for instance, has been taken from a light soil in the cool, dry, even climate of the mountains of Chili and grown for years in all soils in the warm, damp, changeable climate of the British Isles, and, to make matters worse, instead of starting again and again with new individuals produced from seed, the life of single individuals has been indefinitely protracted by purely vegetative reproduction. Part of an older plant, namely the tuber, has been divided up into new plants, and this process has been repeated till, under the artificial conditions of growth and multiplication, the plant has lost all stamina. What wonder, then, that the last fifty years tells a tale of continued struggle between the weakened plant and disease? It is to cross-fertilisation that we are now looking for help in the fight with the fungoid and other pests which have done, and are doing, so much damage. During the last few years Messrs. Sutton have raised seedlings by crossing a variety of the common potato, *Solanum tuberosum*, with another species, *Solanum Maglia*, or, as it is sometimes called, Darwin's potato; and, though it is too early for definite statement, we may hope that in a few years some of the seedlings will prove the precursors of varieties yielding crops good in themselves and also less liable to disease. Not only is there the strengthening influence of the new blood introduced by the cross, but the advantageous fact that *Solanum Maglia* is a native of a damper, warmer climate, more like our own than is that to which the common potato is indigenous.

The recent discovery of seedling sugar-canes may exercise an important influence on the prosperity of our West Indian colonies. As with the potato so with the sugar-cane, vegetative propagation has been exclusively followed: new individuals have been produced, not from seeds, but from cuttings of the joints of older plants. Indeed, so long has the sugar-cane been cultivated that, like the wheat, its native country is to-day a matter of dispute, and it is a question if it is now ever found in the wild state. In consequence, perhaps, of the many centuries of cultivation, the plant has almost lost the habit of producing seeds, and we may search the "arrows," as the long, feathery flower-heads are called, and find not a single fertile flower. It is obvious, that if the ovary has become permanently sterile, or the pollen lost the power of fertilisation, the benefits of cross-fertilisation in the form of invigorated offspring or new varieties, are precluded. Until about three years ago there was a very general opinion, and one, too, of long standing, that the sugar-cane did not seed. In 1889, however, as the result of observations in Java, Dr. Benecke published a description and drawings of the ripe fruit and the germination of the seed; while, almost at the same time, Messrs. Harrison and Bovell, working at the Botanical Station at Barbados, proved that some varieties on the island also produced seed. From them seedling plants were raised, while some seed sent to England was successfully germinated at Kew, and also by Messrs. Veitch at Chelsea. Fertile flowers on the "arrow" are few and far between, and the size is very small; but in spite of these difficulties the discovery opens up the prospect of seminal reproduction, and the advantages of cross-fertilisation.

The object of the cross-breeder is to secure in one variety the valuable characteristics of two or more; to get, in fact, the largest possible crop of the best quality, and ripening at the proper season. In no case is this of more importance than with wheat and other cereals, the cultivation of which in Great Britain is now at so great a discount. In 1839, according to a statement in the *Journal of the Royal Agricultural Society*, the annual growth of wheat in England and Wales was estimated at 12,350,000 qrs., worth, at 50s. per qr., nearly 31 million pounds sterling. Fifty years later, in the same journal, we read: "The present annual growth in the United Kingdom does not exceed 9 million quarters." As the crop is still held to be indispensable in the corn-growing districts, the introduction of improved varieties for the sake of adding to the yield is of great importance. An account by Mr. Carruthers of some experiments on the cross-fertilisation of cereals, published in the latest volume of the same journal, is consequently of great interest, and no apology need be made for a brief re-description in NATURAL SCIENCE.

In the great majority of grasses the male and female organs, stamens and pistil, are contained in the same flower, not surrounded by an attractive corolla of petals but more or less enclosed in

brownish scales called glumes, the number of which varies in different genera. Each flower is solitary, or more often several are crowded together forming a spikelet, while the spikelets may be arranged closely forming the so-called "spike" as in wheat and barley, or loosely as in the "panicle" of the oat.

In each flower the stamens and pistil are generally ripe at the same time and thus self-fertilisation is favoured. The barley is an extreme case, as here the fertilising pollen escapes from the stamen and falls on the moist receptive stigma of the pistil before the flower opens and while the ear is still within the sheath. When the stigmas are finally pushed out beyond the scales of the flower they are dead, and cross-fertilisation is therefore precluded. In the wheat the anthers are already burst and some of the pollen-grains discharged and adhering to the divisions of the ripe, feathery stigma before the flower naturally opens, showing that self-fertilisation is at any rate possible. The flower opens for a short time, about a minute, the opening being effected through the pressure of the stamens, stigmas, and the two small scales, or lodicules, supposed by some to represent the petals; the filaments pushing the anthers before them extend to three or four times their former length, and the feathery stigmas spread to the air. The pendulous anthers are shaken by the least breath of air and rapidly emptied of their remaining pollen-grains, so that the transference of pollen from one plant to the stigmas of the flowers of another is also rendered possible. Observations in the field show, however, that self-fertilisation is the rule, for in experimental grounds where different varieties of wheat are grown in close proximity in parallel rows, no accidental cross-fertilisation due to wind-carried pollen-grains has been noticed.

Artificial cross-fertilisation of wheat or barley is a delicate operation. The parts of the flower are small, and the slender stamens must be removed before the pollen is ripe, while the stigmas must be protected from the access of pollen from outside. The application of the selected pollen to the feathery stigma is best accomplished by applying the ripe grains direct from the anthers to the stigma by gently moving them over its surface. Moreover, this has all to be done in the open field on a flower supported on a long, slender, and easily-moved straw.

The offspring obtained by cross-breeding vary in fertility with the closeness of affinity between the two parents. By fertilisation with pollen from the flower of a similar plant of the same species, or even of a plant with characters distinct and permanent enough to be recognised as a variety, though still of the same species, we obtain offspring producing fertile seeds from generation to generation. When the pollen applied is taken from an allied species of the same genus, perfect seeds can generally be obtained, but the hybrid plant of the next generation is almost always sterile, though the vegetative organs, such as leaf and stem, are often finer. In the case of widely-

separated species of a genus the difficulties are greater, and the chances of procuring fertile offspring less; while hybrids between distinct genera are rare. Out of four hundred experiments in fertilisation of plants of the four different genera—wheat, rye, barley, and oats, with pollen from each other, in only two cases were seeds produced. The two seeds, hybrids between rye and wheat, germinated and produced plants intermediate between the parents, but the pollen did not ripen, and consequently the flowers set no seed.

In the case of plants which are not cultivated for the sake of their seed, but merely for the flower as by the nurseryman, or for some portion of the vegetative structure like the sugar-cane or the potato, the absence of seed is no disadvantage, while the increased vigour of the vegetative organs may be an advantage. It is obvious, however, that with cereals we must keep well within the limits which will ensure production of seed; attempts to produce improved forms must be limited to the crossing of individuals of a single species, including its varieties. Since they have been so long under culture and have given rise to so many cultivated varieties, it is not only hard to determine which variety most nearly represents the primitive form, but also with how many species we are dealing. The following seven types of wheat are easily distinguished:—

1. *Triticum monococcum*, L., with one grain in each spikelet, and the innermost scale (or pale) dividing into two when ripe.

2. *T. Spelta*, L. (spelt), with a loose four-sided ear, and grains which do not fall out in threshing.

3. *T. dicoccum*, Schrank, with dense two-sided ears, and grains as in the spelt.

4. *T. vulgare*, Vill., with compact ears, and grains falling when threshed.

5. *T. turgidum*, L., with dense four-sided ears, and short, thick, blunt grains, which fall out in the threshing.

6. *T. durum*, Desf., with dense long-awned ears, and long, narrow, very hard, and flinty grains, which fall when threshed.

7. *T. polonicum*, L., with long compressed ears, long papery glumes, and long narrow grains.

Botanists hold different opinions as to the number of species comprised by these seven types. Blomeyer unites them under one, *T. sativum*, L., while Hackel, the greatest living authority on grasses, thinks *T. monococcum* must be separated as a distinct species. The second view is supported by the fact that fertile hybrids have not been obtained between *T. monococcum* and any of the varieties of *T. sativum*. Hackel considers *T. polonicum* as a species which has, perhaps, arisen under cultivation. Our illustrious countryman Bentham recognised three species, *T. monococcum*, *T. Spelta*, and *T. sativum*, the last being represented by several varieties. Hence, if we wish to obtain improved varieties by means of cross-fertilisation,

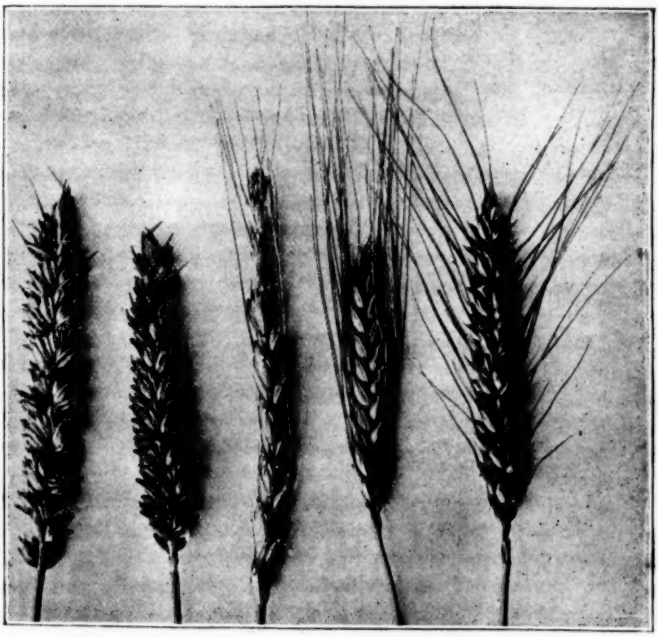
T. monococcum must be tabooed, while *T. polonicum* had also better be avoided.

The ideal wheat for cultivation in Britain should give plenty of straw of good quality, and strong enough to carry the ear till ripe without being laid, should tiller—*i.e.*, produce new shoots from the base—freely and ripen early, yielding a good weight of fine well-filled seeds. "In selecting parents it should be remembered that the male appears to exercise a special influence on the seed, while the female affects the character of the vegetative parts of the plant." In the *Royal Agricultural Society's Journal* for 1889, Mr. Henry Evershed gave an account of experiments on the cross-fertilisation of wheat carried out by Messrs. Carter; the cross-bred seedlings yielded a variety of forms, and careful selection was required to fix permanently the best of these. In his recent paper, Mr. Carruthers describes some results of experiments in cross-breeding, extending over thirty years, made by Messrs. R. & J. Garton, at Newton-le-Willows, Lancashire. These gentlemen have collected numerous varieties from which to select suitable parents, not only from Britain, but from all parts of the world. Parents have been chosen both from the established varieties with which they began and also from the improved ones obtained in the course of their experiments. Whenever a good point in quality or quantity of straw or grain was detected, an endeavour was made to maintain or increase it by using the plant in turn as a parent. Thus the varieties with which they are now working are the products of many previous crosses, and may be termed composite crosses. A serious difficulty exists in the increased tendency to variation with the increase in the number of parents, and careful selection of desirable forms and their continued cultivation is necessary, with persistent elimination of sports and defective plants, before a fixed variety can be established.

Our first figure represents a few of the parents of the new varieties raised by Messrs. Garton. Hardcastle and Mainstay are in general cultivation. The spelt was used in the hope of getting a form which would inherit a seed so enclosed in the glumes as not to fall out during harvesting, while the flinty seeds of the Hard wheat and the abundant cropping of the Grey wheat offered qualities which might be combined with advantage. After several years' crossing, a selection was made of the most hopeful varieties, and these have been under careful cultivation for three years. The second figure represents eleven of these composite crosses grown in 1893, "all of which are the progeny of a selected plant of the harvest of 1890." The remarkable tendency to sport is well shown; it results from the great diversity of ancestors represented in the direct parent, the 1890 plant, and gives some idea of the difficulty experienced in getting a pure and fixed variety. An examination of the individual figures and comparison with the figures in our first illustration will show the effect of the different original parents.

Thus the first figure recalls both Hardcastle and Mainstay, but the ear has more spikelets, each of which contains an increased number of larger grains. The spelt comes out very strongly in Nos. 3, 4, and 5; the last approaches it most nearly, but has larger spikelets more compactly arranged on the main axis. Nos. 3 and 4 contain characters derived from the commonly cultivated varieties. No. 6 is somewhat similar, but branched. In this case the axis of the spikelets, after producing two normal seeds, has, instead of bearing two or three additional flowers as usual, become extraordinarily lengthened, forming a secondary axis which in turn bears smaller spikelets. A similar

FIG. 1.—SOME OF THE VARIETIES USED AS PARENTS.



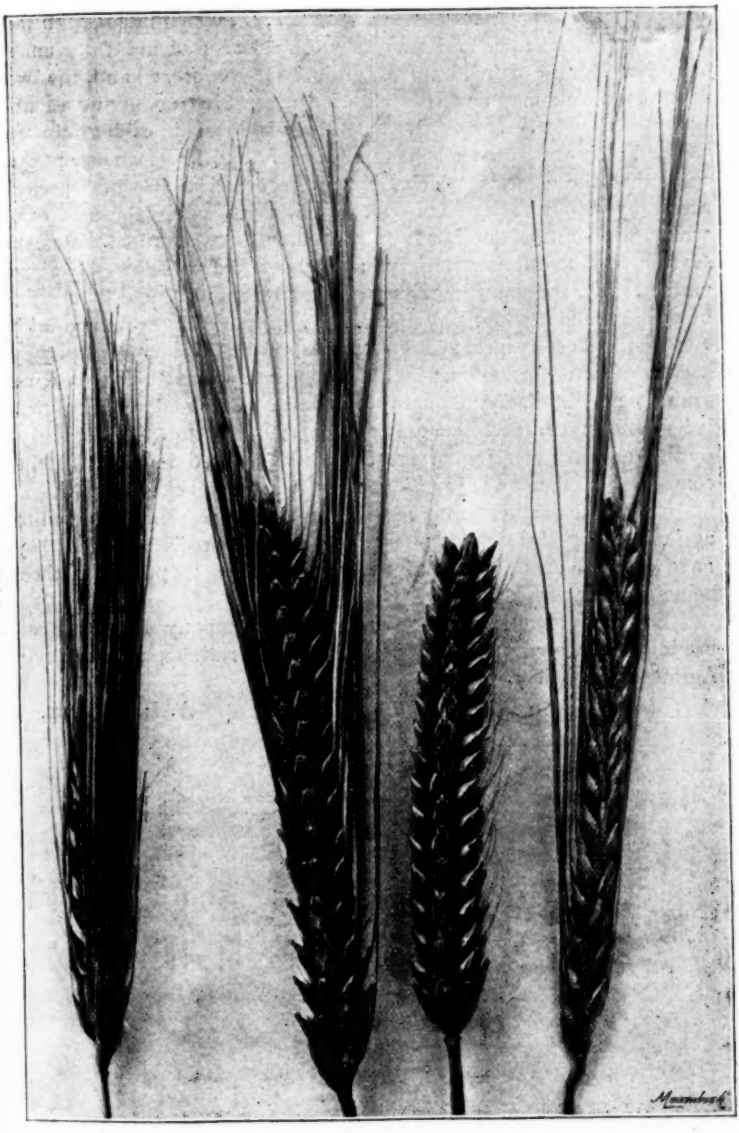
Hardcastle. Mainstay. Spelt. (*T. spelta.*) Hard Wheat. (*T. durum.*) Grey Wheat. (*T. turgidum.*)
Reduced One-half Natural Size.

monstrosity in the Grey wheat has produced the branched variety known as Mummy wheat (*T. decompositum, L.*) In No. 7 the spelt is again in evidence, though influenced by a bearded form. In Nos. 8 and 9 is seen the effect of the Grey wheat, as also in No. 11, which is, however, nearer the selected Bearded wheat.

The different forms of cultivated barleys are probably all varieties of a single species. The spikelets are borne in groups of threes on the opposite sides of the axis, but as a rule only the centre one of each group is perfect and produces seed. The ear is thus two-rowed

1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11.

FIG. 2.—COMPOSITE CROSSES, PROGENY OF THE SAME PARENTS IN THE THIRD YEAR OF CULTIVATION.—*Reduced to Two-fifths Natural Size.*



1. 2. 3. 4.

1.—Bere (Parent). 2 and 3.—Crosses (Progeny). 4.—Two-Rowed Barley (Parent).

FIG. 3.—PARENTS AND CROSS-PROGENY OF BARLEY.
A little less than Natural Size.

(distichous), while the barren spikelets from opposite groups are placed close together along the centre of the flat face of the ear, hiding the axis. This characterises *Hordeum distichum, L.*, which includes all the two-rowed forms; the ears may be white or coloured, and the glumes may be adherent or free from the grain. On the other hand, the two lateral spikelets may be fertile and the central barren, giving a four-rowed variety (*H. vulgare, L.*). If all three spikelets in each group are perfect, a six-rowed ear results (*H. hexastichum, L.*). If we accept the view that these forms, which Linnæus regarded as distinct species, are really merely varieties of one, we see a reason for the ease with which cross-fertilisation has been carried out between them. Messrs. Garton tried to obtain a variety in which all the spikelets should be perfect, while the size and quality of the seed should equal that of the best cultivated two-ranked forms. By crossing a six-ranked with one of the best two-ranked varieties, known as Golden Melon, they succeeded, after further crossing, in getting composite varieties bearing grains equal in size and quality to those of the Golden Melon, but, being six-ranked, three times more numerous (see Fig. 3). Similarly, by the repeated crossing of the three well-marked forms of oats, a considerable quantity of luxuriant new forms have been obtained.

As Mr. Carruthers observes, the important services rendered to agriculture by these gentlemen will be more appreciated when they have secured enough seed of the new varieties to permit of their being grown as farm crops.

For the interesting illustrations which accompany this article we are indebted, through the kindness of Mr. Carruthers, to the Royal Agricultural Society.

A. B. RENDLE.

IV.

Neuter Insects and Darwinism.

MR. PLATT BALL (5) does not think that Herbert Spencer's reply (2) to Weismann's argument concerning neuter insects, in the *Contemporary* controversy, is convincing, and he adduces certain new points to which he thinks Spencer's arguments do not apply. I propose to examine again the questions in dispute, and to try to ascertain how much weight is to be attributed to Mr. Ball's contribution to the discussion. I do not propose to take the question of use-inheritance as my chief point, but to consider what are the most logical conclusions we can draw from the facts before us as to the explanation of the phenomena presented by social polymorphic insects, or those in which several forms are produced by one parent. As I have no original knowledge of these insects, I intend to take the facts chiefly as they are accepted and presented by the three disputants above mentioned.

Weismann (1) limits himself to the phenomena presented by ants. He says that it may be taken for granted that the ant workers have arisen through phyletic metamorphosis of fruitful females. What other origin could they have had? he asks. As evidence of this he mentions that to this day there are some species in which the workers closely resemble the females, and that in other cases intermediate forms have frequently been found. This is the most extraordinary assumption with which to begin his argument, for the one thing obvious and certain is that the ant workers have not arisen by the phyletic metamorphosis of fruitful females. The very fact that the workers are sterile precludes this assumption. They are the daughters and sisters of fruitful females, not the offspring of parents possessing their own peculiarities. No animal can be said to have arisen by phyletic metamorphosis from its mother; and as all the ancestors of the neuter ants were fruitful females, it is difficult to see where the phyletic metamorphosis comes in. None of these ancestors have exhibited degrees of modification which lead to the peculiarities seen in the workers.

Whether the natural selection of females which have possessed in different degrees the property of generating workers adapted to their work is the true explanation of the evolution of the workers, that is the question I am about to consider. But to call this hypothetical

process the phyletic metamorphosis of fruitful females is simply a misuse of terms. Phyletic metamorphosis means the gradual change occurring in a series of organisms of which each is the offspring of the preceding. The origin of the neuter social insect from the perfect female is immediate and sudden. In fact, it is not what has always been understood as phyletic metamorphosis, but what is commonly and universally known as sexual reproduction.

But the problem is why is the neuter insect different from its mother, instead of being similar to her? Mr. Spencer has given the obvious answer which every biologist might have been supposed to know, namely, that the larva of the neuter insect is underfed, an explanation which is ignored by Weismann throughout his article. There are various instances which support the conclusion that in all cases the peculiarities of the sterile persons are due to the conditions to which the larva is exposed, but the one thoroughly-established fact is that the hive-bees do actually produce perfect queens out of any worker-egg or worker larva taken at random. This is sufficient proof that the difference between a worker and a queen is not a constitutional difference, but a difference due to the conditions of development. Without at present going into the theory of biological determinants, it must be admitted that peculiarities which are hereditary and to be regarded as evolved are predetermined in the egg. The fact above-mentioned concerning the bees, as well as other similar facts to which Spencer alludes, show that the peculiarities of neuter insects are not predetermined in the egg, but related to certain definite conditions of development. They are not, therefore, either inherited or evolved. It may be said that we do not know this for ants, but at least we know nothing to the contrary.

It follows from this that Weismann is not justified in describing the differences between the worker ant and the perfect female as retrogressive and progressive, implying and maintaining that these differences have been gradually evolved. Among the instances of retrogression he refers to are the absence of the receptaculum seminis, and the reduction in the number of egg-tubes, a reduction which appears in different degrees in different species of ant, the persistent tubes numbering 12, 5, 3, 1, or none at all. It is clear that since the larval ant has the inherent potentiality of perfect generative organs, the reduction in the worker is not due to degeneration but to arrested development. It may be maintained that certain qualities in bullocks can be produced by the selection of bulls and cows, not by the appearance of these qualities in themselves, but entirely because they breed the kind of bullocks required. But no one will maintain that the sterility of the bullock is one of the characters that have been so produced. It makes no difference to the argument that in one case the testes are removed by amputation, in the other the ovaries are not fully developed. The cases are similar in the predetermination of the organs in the ovum. Supposing a

certain number of young males in a herd of deer were regularly castrated, it could not be said that a caste of stags without antlers had been evolved by natural selection or by phyletic metamorphosis.

Other cases of retrogression mentioned by Weismann are the reduction in the number of facets in the compound eyes and the absence of wings. It is obvious that here again we must hold, until the contrary is proved, that the organs fail to develop in consequence of the insufficient nutrition of the larva, and that the eggs in respect of their potentialities or "determinants" are all alike. It is well-known that in true cases of phyletic degeneration there is in the majority of instances actual retrogression in the course of individual development, as, for example, in the cases of the teeth and hind limbs of whales. No evidence has been produced that anything of the kind occurs in the development of the neuters of the social insects.

A very important factor in the peculiar polymorphism of these insects is the phenomenon of insect metamorphosis. The wings and other important organs of the perfect insect are developed not continuously, but within a certain brief period, *i.e.*, during the pupa stage. There is good reason for the conclusion that the conditions to which the larvæ are exposed effect definite differences in the metamorphosis. It is known that temperature alone produces definite results in the colours of certain lepidoptera, that is to say, the temperature to which the larvæ are artificially exposed. Weismann says that the changes in the thorax of the worker ant are just those which would arise through transmission of the deteriorating effect of disuse, but the workers are sterile. He ignores, what is much more important, that the changes are just those which would arise from incomplete development in the metamorphosis, and the worker larvæ are reared in conditions which are known to lead to such incompleteness in the metamorphosis.

Are we to be told, then, it will be asked, that the workers have always been exactly as they are now; have had their peculiar differences fully expressed since the first moment when the solitary ancestor began to be social? To which I reply that the conditions under which the worker larvæ are reared may have been gradually diverging from the conditions required for the production of the perfect insect, and the constitutional peculiarities of the perfect insect have probably undergone gradual modification, but the differences between the worker and the perfect female have always been the direct result of the differences in the conditions under which the larvæ were reared. There is no reason, so far as minus differences in structure are concerned, to suppose that the females have acquired by variation and selection the constitutional property of producing workers of a certain kind. There is no reason to suppose that in the beginning the eggs of the female ant could not be developed into wingless neuter ants as they are now.

Let us now turn to the alleged progressive development of the workers, and consider whether the argument of the selectionist is any stronger in regard to these. The first instance cited by Weismann is the great increase in the brain. The females, never having had any brains to speak of themselves, are alleged to have acquired, as the result of selection, the property of generating workers with large brains. In the first place, I would point out that I know of no careful comparison between the brains of a newly-emerged worker ant and a newly-emerged queen. It is in accordance with well-established facts to conclude that after emergence the brain of the worker ant grows larger, while that of the perfect female diminishes, simply as a result of use and disuse in the individual. Spencer maintains that apparent gains on the part of the worker are really due to the persistence of organs and characters which have diminished in the fertile insects. I am not prepared to say that these two principles fully explain all the apparent plus modification exhibited by sterile workers; but I do maintain that no sufficient evidence has yet been brought forward to show that constitutional characters are present in the workers which have been evolved in them alone, and are neither due to the action of conditions on the individual development nor to inheritance from the original ancestors.

Weismann lays great emphasis on the enlarged heads and jaws in the so-called soldier caste in certain species, such as *Pheidole megacephala* and *Colobopsis truncata*. But it must be pointed out that the very fact that this character distinguishes a special caste of workers is difficult to reconcile with the explanation upheld by Weismann. For, as Lubbock says, the question arises whether those different kinds of workers are produced from different eggs. Lubbock (3) speaks from special and original knowledge of these insects, and he is disposed to concur with Westwood, that the inhabitants of the nest have the instinct so to modify the circumstances producing this state of imperfection, that some neuters shall exhibit characters at variance with those of the common kind. Considering the established fact concerning bees, it is by no means an extravagant suggestion that where there are two or even more castes of workers, the eggs are all essentially similar, and simply developed under definite differences of conditions. If this is the case, and the presumption is that it is, then there is no evolution of the castes in the sense of the modification of the characters predetermined in the eggs. It must be remembered that in all these polymorphic social insects the larvæ may be said to be artificially developed. The conditions in which the larvæ live are contrived by the full-grown workers, and are kept wonderfully constant. We cannot say, as even Weismann would admit, that the small feet of Chinese ladies are due to phyletic metamorphosis or to Natural Selection. Their growth is moulded and checked by the unyielding influence of material bonds, and we have good reason to suppose that equally rigid external conditions mould the developing larvæ of ants,

although Lubbock assures us that the exact mode by which the differences are produced is still entirely unknown.

Into what obvious fallacies Weismann is capable of falling in consequence of his over-zeal for his own views is shown by two cases which I will cite from his first article in the *Contemporary*. To prove that pammixis alone suffices to bring about the complete disappearance of characters, he adduces the degeneration of the feeding instinct in *Polyergus rufescens*, which starves to death unless crammed from the crop of a slave-ant. He states correctly that not only the males and females, but the workers also in this species, have altogether forgotten how to recognise their food. He says that this would be an excellent example of the transmission of functional degeneration—"if only these amazons were not sterile." But, of course, the males and females are not sterile, and Weismann is inadvertently aiming weapons at himself. Another curious fallacy in the same article is the reference to the skin armature of hermit-crabs as one of those passive organs which cannot be strengthened by use, and yet which disappear when they cease to be useful. This case seems so convincing to Weismann that he can only suppose that Spencer ignores it because the philosopher is unacquainted by personal observation with facts so familiar to the naturalist. And yet the wonder really is that Weismann should ignore the fact that the rigid cuticle of crustacea is not merely an armour of defence, but an external skeleton to which the muscles are attached, and which is necessary to their movements. Every naturalist ought to know that in the tail of the hermit-crab the muscles and the cuticle are equally degenerate, have diminished *pari passu*, except certain appendages forming the apparatus at the end of the tail by which the hermit holds on to the shell he inhabits, and in this apparatus both the muscles and the cuticle are well-developed. In this case the weapon so confidently aimed at the despised philosopher bursts in the naturalist's hands, and does more damage to himself than to his adversary.

I have not hither to dealt with the question of habits and instincts. It is confidently held by selectionists like Mr. Platt Ball that the wonderful instincts of the sterile forms of social insects could never have been present in the ancestral perfect insects, and can only be explained by the natural selection of parents which had the power of producing workers that possessed them. Spencer maintains that the instincts are inherited from ancestral perfect insects, in which they were developed by habit and practice. That this is largely true there can be little doubt, when we consider the numerous intermediate cases between solitary females and the most highly-developed communities. What remains, it seems to me, is easily explained as the result of individual education. It is certain that the newly-emerged worker wasp begins making cells and feeding larvæ in imitation of her mother, having no sexual instinct and no males to mate with. The worker wasp inherits much directly from her mother, although not sexual instincts, and learns much by imitation. It is true that

Spencer probably falls into error in deriving the swarming of bees from a nuptial flight, because the nuptial flight takes place separately in a different manner. But it must be remembered that it is the old female which leads out a swarm, and her successor leads out the next, and so on. It is well-known to all experts in bee-keeping, and is stated in the manuals, that the real cause of swarming is simply want of room. The cells are full either of honey or larvæ, and there is not room for any more combs. The workers force the old queen to come with them to find a new home. The story is that they prevent her from destroying the young queens, in order that the old hive may not be left without a queen. But in most that has been written about bees, there is a great deal of teleology and anthropomorphism, which it is impossible to accept without scepticism. The fact is that the workers have one set of instincts, to collect honey, build cells, and rear the progeny of a perfect female. When there is no more room to do this in the cavity they inhabit, they take the female to another cavity. It is practically the custom among skilled apiarians to prevent swarming by simply adding super-hives.

We see, therefore, that although Mr. Spencer's suggestion that swarming is to be regarded as a modification of the nuptial flight leads to difficulties which Mr. Platt Ball rightly holds to be insuperable, it has not been shown that the present condition of the hive bee can only be explained by the natural selection of indefinite variations. The true explanation can only be reached by regarding the structures and habits of bees, not as advantageous, but necessary, that is to say, as the necessary results of conditions. Mr. Ball argues that the special instincts commenced in fertile daughters who helped the queen-mother at the cost of delay or neglect of their own maternal functions, a step towards the formation of a neuter caste. It seems to me impossible to believe anything of the kind. It is much more reasonable to believe with Mr. Spencer that the workers looked after the eggs and larvæ of the perfect female simply because they were themselves sterile. They were sterile through imperfect nutrition in the larval state, and, therefore, having no sexual instincts, but retaining the maternal, they proceeded from hereditary instinct and from imitation to rear their brothers and sisters. No one can deny that it is the queen or perfect female which has been modified rather than the workers, in view of the fact that the pollen-brush on the legs is entirely wanting in the queen bee, though well-developed in the neuter, while in every other species the pollen-brush is present in the perfect female. Are we not entitled to regard this as evidence of the transmission of the effects of disuse? The female bee transmits the pollen-brush to her imperfect descendants in every generation, simply because they remain permanently at an earlier stage of development. No doubt the pollen-brush would be found to be represented as a transitory stage in the development of the queen bee, if the history of her metamorphosis were completely traced.

Anyone who candidly and thoughtfully considers the views I have tried to express will see that Mr. Platt Ball's objections are superficial difficulties due to zeal for the dogma of natural selection. In science everyone should take as his watchword *surtout point de zèle*. It is true we cannot maintain that the honey-pot modification in ants was originally possessed by perfect males and females. But why should we believe that the peculiarity is inherited at all? There is nothing unusual in ants or bees regurgitating honey, feeding one another or larvæ. A differentiation of habits must ensue when a number of neuter insects live together in a community, simply because all the individuals cannot be similarly related to their conditions and their work. Mutual relations are established between them, and those persons which remain in the nest, receive food, and give it back again, may well, when food is plentiful, become distended. The fact that one of these honey ants lives in Mexico and one in Australia shows that the peculiarity has arisen twice independently. It is not yet perfectly certain that the persons with the distended abdomens are a separate caste: every individual may be at times in this condition.

Mr. Ball's statement of the case of the termites turns out to be a characteristic anachronism. Like a good many other faithful adherents of the theory of natural selection, he seems to have neglected to take advantage of sources of information more recent in date than the "Origin of Species." Not having made a special study of polymorphic insects, I found it difficult to obtain original papers containing the results of research on these social neuroptera. I had a strong conviction that when the subject was properly investigated it would be found that the different castes were reared under different conditions. At last I find that, according to Emery (4) the investigation has recently been carried out, with the results which I anticipated. Grassi, an Italian biologist, has satisfied himself and published direct evidence that the termites have the power of regulating the number of workers and soldiers by rearing them at will by appropriate feeding and treatment, as they also hasten the sexual maturity of other individuals by special nourishment, and so produce the supplementary sexual individuals which were so long a mystery. Emery concludes that the workers of all social insects are reared in a similar way from germs which are capable of producing normal sexual individuals.

Mr. Ball's last objection as to the evolution of sterility in neuters is a mere bladder which scarcely needs special pricking. It has been disposed of by the general position taken up by Spencer and myself. The only reasonable view is that the sterility of workers is the necessary result of the habit of the ancestral insect to bestow maternal care on her young. Is it not a fact that in the human species the eldest sister in a large and struggling family has her chance of marriage reduced? The characteristics and the usefulness of old maids in the human species are familiar enough; does anyone

suppose that they are the result of the phyletic metamorphosis of fruitful females? Weismann triumphantly adduced the case of neuter insects in social species as an incontrovertible proof that natural selection could do every thing without the possibility of any transmission of somatogenic or so-called "acquired" characters. As in many other instances, the argument is produced by looking at the phenomenon upside down. I believe that some day it will be generally admitted that the whole natural selection doctrine is, in the etymological sense, preposterous. There was a time when it was universally held that the sun moved round the earth. So it does, apparently. It has a motion across the heavens from east to west in relation to a human observer. The belief was true up to a certain point. But Galileo discovered that the earth moved round its own axis. In relation to the whole solar system the sun is fixed—the earth moves. So it is with the evolution of organisms. Assuming the occurrence of the modifications, it may be said from an anthropomorphic point of view that natural selection preserves them. But it is a much deeper truth that evolution is the result of successive modifications or changes in succeeding generations, and the mere survival of these modifications is not the cause of them. Whatever may be the case in other instances, it is clear that the peculiarities of neuter insects as compared with the perfect individuals of their species, are the direct result of the conditions of life. Under similar conditions the eggs all develop into similar individuals; the differences of the individuals correspond to the differences of conditions. The differentiation has doubtless been progressive, because the differentiation of conditions has been progressive. If we start from a solitary perfect female which had developed the habit of bestowing maternal care on her young, the gradual development of the system naturally follows. There is no reason to believe that the arrest of the metamorphosis by insufficient food, etc., has been evolved specially in the social insect. It occurs in the Axolotl, and also in the common tadpole; in the former naturally, in the latter artificially. The social insect is not a community, it is a family. The queen is merely a breeding mother. As soon as she began to feed her larvæ piecemeal, instead of laying an egg in a store of food, it was inevitable that some of them should be inadequately nourished. These by instinct and imitation fed their brothers and sisters, and when food was abundant perfect insects were produced. But the mother having less work to do and more food, became increasingly fertile, so that in subsequent generations more workers were produced, and she had still less work to do. So long as the perfect female started in spring alone without help she continued to perform maternal duties herself. Where she survived the winter with a number of workers, the need for work on her part ceased. As soon as the workers existed the relations of these and the female to the outer world were different. The consequent modifications of the perfect female were transmitted

to her descendants. Thus the workers also change, for though the eggs of one female are all alike, the imperfect development of these eggs may well result in a different worker now from that which was produced in previous generations. When the female with some workers survived the winter, her specialisation went much further, for after this she never worked at all. In considering how this change took place in the hive bee, we must remember that this form was originally domesticated in a warmer climate. It is not native to Britain in the wild state. In the case of ants and termites, the continuity of the community could probably be easily explained from their fossorial and constructive habits, which prevent their destruction by the seasons. It has been proved by Lubbock that a single female can originate an ant community, although she probably does not usually do so. The termite king and queen appear to be too much specialised for reproduction to found a community without the help of workers. So far, then, from being a triumphant proof of the all-sufficiency of natural selection, the peculiarities of social polymorphic insects, rightly regarded, offer the strongest support to Lamarckian principles. It may be said that the degeneration of working organs and instincts in the so-called queens, and their increased fertility, were advantages to the species and therefore survived, but it seems to me that it is difficult to avoid the conclusion that these modifications are the direct result of disuse and stimulation, and that they are transmitted.

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V.

Continental Growth and Geological Periods.

IN the early days of Geology a period indicated a certain section of the earth's history distinctly marked off from that which preceded and that which followed. Each period had a fauna and flora peculiar to itself by which it could be recognised through the fossil remains found embedded in its rocks. There is no doubt that the Mosaic account of the Creation gave traditional support to the notion of distinct breaks in the geological chain. Each period represented not unnaturally to early thinkers a separate creation followed by complete destruction. What was first noticed were the salient differences between the fossil contents of strata geologically far apart, such as the reptiles of the Lias and the fishes of the Chalk. Ingenious men found then, as Mr. Gladstone does now, a parallelism in the order of creation between the Mosaic account and the record of the rocks.

When, however, the record was further searched, interesting links were discovered, which, if they did not actually bridge over the differences, led men to think that, could all be restored, the earth's history would be found to be one continuous record unbroken by cataclysmic collapses and successive repairs. Curiously enough, these ideas held their ground in face of the uniformitarian theory of the earth given to the world by Hutton at the close of the last century. Lyell, following on the same lines, with a wealth of illustration and rare literary skill, showed that a true interpretation of nature in the past was to be sought in the action of present causes. The science of geology was thus put on a stable base, and men were taught to arrive at their opinions by reasoning upon facts instead of merely giving free scope to their imaginations. The doctrine of uniformity may not be theoretically correct; indeed, uniformitarianism is a misnomer even as applied to Lyell's conception of the history of the earth. It might with quite as much justice be called development. The popular conception, however, of uniformity was that things are now as they have been in the past and as they will be in the future. From this it resulted that "periods" came to be looked upon only as arbitrary divisions of geological history which enabled one to grasp the sequence of geological events. Indeed, it is but fair to say that there is not in the whole of Lyell's "Principles," for which no

one has a profounder regard than myself, any indication of how the distinctive geological and physical features of the several periods came about, or, indeed, why they should exist at all. As a student of Lyell from my earliest dabblings in geology, this was long a perfect mystery to me. In putting forward the following suggestions as to how the periods were evolved, I do so with all humility, looking upon them as a development of the great master's work.

Sedimentation and Land Making.

It is a well-known axiom in geology that the land is being lowered at an average rate of about 1 foot in 4,000 years by meteoric action, by rain and rivers, or all those chemical and mechanical forces that come under the general term subaërial denudation. To this is to be added the mechanical abrasion of coasts. The matter carried into the ocean in solution in river waters is, I have shown on the average of many years, about one-third that in suspension. This seems at first blush a large proportion, but when we consider that the matter in solution is a much more constant quantity than the matter in suspension, our surprise is modified.

The sedimentary matter—which is chiefly silica either in the form of grains of sand, or in a much finer state of comminution, as flour of rock mixed with the decomposition products of various rocks, notably felspathic, forming what, when deposited, we call clay—is laid down in a more restricted area than the matter in solution. Mixed with these products of denudation are calcareous particles, mica and other minerals, which all go to make up one or other of the various sedimentary strata. These are, as a whole, often looked upon as the effects of mechanical erosion, but, so far as this is an expression of dynamic action, they are only partially so. Chemical forces have, in my opinion, much more to do with loosening the bonds of the rocky particles than has mere pounding of the boulders along shores and river-beds; they also effect the separation of the rock-masses. To be impressed with this fact one has only to look at some of the enormous masses of rock in mountain districts in Wales, moved to their present positions during the last phase of the Ice-age. Although they have been exposed to nothing more than meteorological influences since, they are frequently split up into many separate blocks and are much weathered. Likewise, in granitic districts enormous masses of granitic sand are, as we may say, liberated by the decomposition of granite.

To these sediments must be added boulders and pebbles which go to form conglomerates. These are, however, seldom delivered into the ocean by large rivers. They remain in the higher reaches or mountain tributaries, so that the boulder-beds found in the sea are either the products of coast erosion, the dynamic undermining of cliffs by wave action assisted by meteorological influences, or are formed by mountain torrents with swift gradients, or are carried by glacial agency or by floating ice.

These mechanical products of denudation are sifted out and arranged by the ocean waves, tides and currents, in the order of their sizes and specific gravities, so that the boulder-beds are mostly beach deposits, while the pebbles, gravel, grains of sand, and finely-triturated material are carried out further and further from the coast and into deeper water in the inverse order of the size of the particles.

It is well known that the thickest deposits accumulate nearest to the land-masses; I do not indeed think that any fixed line or margin can be drawn, within which limit deposits may be considered terrigenous and outside oceanic, but it is a general principle to be kept in mind. When coast waters are shallow and what is called the continental sub-aqueous plateau extends far out from land, then these mechanical sediments will under ordinary circumstances extend the furthest.[1] But in the extremely interesting "Reports on the Dredging operations off the west coast of Central America to the Galapagos, to the west coast of Mexico and the Gulf of California,"[2] by the U.S. Fish Commission steamer "Albatross," Alex. Agassiz says, "I was struck while trawling on our second line between the Galapagos and Acapulco to observe the great distance from shore to which true terrigenous deposits were carried. There was not a station there occupied of which the bottom could be characterised as strictly oceanic." . . . . "A very fine mud was the characteristic bottom we brought up, often very sticky, and enough of it usually remained on the trawl even when coming up from depths of over 2,000 fathoms materially to interfere with the assorting of the specimens contained in our trawls"; along with the mud, logs, branches of trees, twigs, and decayed vegetable matter usually came up. The distance of the Galapagos Islands from the nearest land in South America is between 500 and 600 geographical miles, while the line between Acapulco and the Galapagos is about 1,100 miles.

The 2,000-fathom line often comes within 100 miles of the coast. Doubtless opposite to the mouths of great rivers, such as the Amazons and the Congo, terrigenous deposits will have a wide extension over the ocean floor. The Indus and Ganges spread their deposits over 700,000 and 900,000 square miles respectively. ("Conditions of Sedimentary Deposition," p. 500.)

There is, however, no doubt that the matters in solution have a much wider extension, only limited, in fact, by the area of the ocean itself. These matters are removed from the water mostly by organic agencies and consist, in the largest proportion, of carbonates and sulphates of lime. Whether any direct precipitation of the matters in solution takes place in the ocean bottom is a subject of surmise, but

[1] *See* "Conditions of Sedimentary Deposition," by Bailey Willis, *Journal of Geology*, vol. i., no. 5, p. 498.

[2] Bulletin of the Museum of Comparative Zoology at Harvard College, vol. xxiii., 1892.

there are grounds for believing that they may be chemically deposited as well as organically separated.

Orographic Relation between Mountain Ranges and New Land-areas.

Leaving for future consideration the regional variations of level which occur in the earth's crust like slow pulsations, and which do not appear to be directly connected with sedimentation, we will briefly examine the evidence pointing to the relation between mountain ranges and new land areas. It is a pretty-well-established fact due to the labours of Hall, Le Conte, Dana, and numerous other investigators, that mountain ranges are built up out of great thicknesses of sediment. Upon this phenomenon is based my theory of the origin of mountain ranges by sedimentary loading and cumulative recurrent expansion.[3] The evidence that mountain ranges are composed of great thicknesses of sedimentary rocks, often with very little unconformity between the rock-groups of which they are built up, is an open book to anyone who takes the trouble to carefully examine the sections, maps, and descriptions of any of the known mountain areas in any part of the globe. It is true of the Rocky Mountains, the Andes, the Himalayas, the Alps, the Caucasians, the mountains of the Turco-Persian frontier as also of the older chains such as the Appalachians and Urals, which have been greatly denuded. It will no doubt turn out to be equally true of the Thian Shan and the great ranges of Central Asia bordering Chinese territory; but these have been so far very little studied.

An examination of the excellent geological map of the world recording all the information up to date, which the labours of Jules Marcou have given us, will show that the rocks comprising the newer mountain chains, which are generally considered to have been upheaved in Tertiary times, have a wide extension beyond the limits of the main mountain-masses. Roughly speaking, the Cretaceous and Tertiary may be said to occupy the half of Europe and a large area of Northern Africa, together, doubtless, with the bed of the Mediterranean Sea. It is just where these rock-groups are most developed and underlaid by more or less conformable Mesozoic groups of great thickness that the mountain-masses occur. This is true even of the Apuan Alps as shown in Stefani's excellent sections.[4] These rock groups appear to extend from the Caspian to the Himalayas, but further eastward we get largely into the unknown.

In North America, though we have not such full information as in Europe, the Cretaceous appears to occupy, or did occupy (having been

[3] "Origin of Mountain Ranges"; London, 1886.—*Also see* Outline of Mr. Mellard Reade's "Theory of the Origin of Mountain Ranges," *Phil. Mag.*, 1891, pp. 485-496.

[4] "Le Pieghe dello Alpi Apuane, contribuzione egli studi sull' origine delle Montagne." Firenze: 1889.

denuded from large areas), about half of the continent, and the same may be said of South America, though in both continents large areas remain to be geologically mapped. It must be understood that I am merely speaking in the rough, to convey the idea I wish to impress upon those who read this, and subject to future correction.

Though there are fresh-water and fluviatile deposits among these rock-groups, especially in the Tertiary, the greater bulk are of marine or estuarine origin. There are also great tabular masses of igneous rocks[5] especially in North America, and volcanic action has over all the areas named played a prominent part from the Tertiary until recent times.

Although there is such a development of Cretaceous and Tertiary rocks on the land areas, there is no doubt a great deal, we cannot say how much, below the sea. The West Indian Islands give evidences in their fauna of a former land-connection with South America,[6] and it may be that the whole area of the Gulf of Mexico and the Carribean Sea is occupied by the same deposits overlaid by great thicknesses of post-Tertiary accumulations. There is little doubt that much of the sea-bed of the Behring Sea and the Aleutian Islands, together with a strip of the sea-bed (none can say how wide) along the western coast of North America, was land in Pleistocene times, as proved by the discovery of mammoth remains on Pribilof Islands and the N.W. coast of America,[7] as also on Santa Rosa, one of the largest of the coast islands of California[8]; and it is not improbable that Cretaceous and Tertiary rocks may occupy much of this area also.

Without traversing the whole of the known globe and making incursions into the China Seas, Malayan Archipelago, New Guinea, New Caledonia, Australia, and New Zealand, it may be well to mention that Tertiary rocks are found far to the north on the land bordering the Arctic Seas.

It is in these Tertiary areas that the greatest mountain chains of the world are situated, and it is further conceded by most geologists that they are the highest because they are the newest and have not

[5] *See* "Report on the Geology of the High Plateaus of Utah," 1880, U.S. Geographical and Geological Survey of the Rocky Mountain Region, by Captain C. E. Dutton; *also*, "A Geological Reconnaissance in Southern Oregon," by Israel Russell. 4th Annual Rept. of the U.S. Geol. Survey.

[6] *See* letter No. 3 by Alex. Agassiz to C. P. Patterson, on the Dredging Operations of the U.S. Steamer "Blake" from 1878–9. *Bull. Museum of Comparative Zoology*, Harvard College, p. 299.

[7] Dr. Geo. Dawson, *Q.J.G.S.*, vol. l., pp. 1–9, 1894.

[8] "The Flora of the Coast Islands of California in Relation to Recent Changes of Physical Geology," J. Le Conte, Bulletin viii., California Academy of Sciences, pp. 515–520. Extraordinary fluctuations of level are also recorded in the successive sea cliffs up to 1,200 feet above sea level in the island of San Clemente off the coast of Southern California in Pleistocene times. *See* "Post-Pliocene Diastrophism of the Coast of Southern California," by A. C. Lawson, Bulletin of the Department of Geology, University of California, 1893.

been exposed to such long-continued denudation as the Appalachians and Urals, for instance.[9]

Establishment of New Land-areas Connected with Mountain Upheaval.

It is evident from the foregoing considerations that the establishment of new land-areas, whether as continental additions or otherwise, is closely connected with mountain upheaval: the two go together. As I have shown, mountain ranges are never formed on the sites of old denuded land-areas, they never arise in the middle of a continent, but are always preceded by great sedimentation. An old continent may subside in the middle or at the margin, and the inland seas or other submerged areas receiving the sediment may in process of time be upheaved vertically and folded horizontally by lateral pressure, and so become eventually an integral portion of the old land. In my "Origin of Mountain Ranges" I have sought to show that this movement is due to expansion, the initial cause being the heating of the sediments and undercrust by the rise of the isogeotherms or surfaces of equal temperature brought about by the accumulation of sediment. It is unnecessary to explain this principle here, as it can be studied in the original work, but I am pleased to point out that Mr. W. J. McGee adopts this theory in his "Memoir on the Pleistocene History of North-East Iowa" (eleventh annual report of U.S. Geol. Survey, 1889-90, pp. 351) with the suggestive addition, since that the sediments rest on inclined surfaces and are themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have moved seawards.[10] Sedimentation, land-making, and mountain building, we have every reason to believe, are directly related in the chain of cause and effect.

[9] "Contributions to the Study of Volcanoes," Judd, *Geol. Mag.*, 1875, pp. 143-152. "The Geological History of some of the Mountain Chains and Groups of Europe," Ramsay, *Mining Journ.*, 1875.

[10] Mr. McGee says:—"The sediments of the successive mantles wrapped about the young continent were dropped from the cold waters of deep ocean, and were long chilled by contact with the waters; yet by the end of the Silurian they were so deeply buried as to no longer feel the chill waters, and to be heated by conduction from the earth's interior; and so the isogeotherms, or planes of equal temperature, ose from the former ocean floor into and through the lower sediments, and their temperature was greatly increased. As they were heated they expanded; a part of this expansion was vertical, and so peripheral portions of the continent were elevated, not by the building up of successive sheets of sediment, but by their own expansion; yet a considerable part of the expansion must have been horizontal, and must have resulted in either mass or particle movement, or both combined; and since the sheets of sediment rested on inclined surfaces and were themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have crept seaward, while the strata at some distance from the shore line must have been compressed horizontally, perhaps buckled and crumpled, and thrown into anticlinal and synclinal folds concentric with the shore, or elsewhere crushed and broken into fragments along the planes of least strength."

STABILITY OF CONDITIONS INDICATED BY THE ACCUMULATION OF GREAT THICKNESSES OF SEDIMENT OVER CERTAIN AREAS.

The accumulation of great thicknesses of sediments extending over millions of square miles, such as took place in Cretaceous and Tertiary times over what is now the western half of the North American Continent, points to an extraordinary constancy of conditions over a vast period of time. Greater than this, perhaps, is the record of the earth's history within the Appalachian chain at an earlier period ending with the close of the Palæozoic. We will, however, for the nonce, confine ourselves to the first-named area, in which are situated the Rockies, the Cascades and Coast Ranges, extending with their analogues through 60 degrees of latitude from Central America to Alaska. It is next to impossible with our present lack of information, though the geology of the North American continent has progressed with rapid strides, to sketch out the disposition of land and sea which endured through the Cretaceous and Tertiary.

It is highly probable, however, that the old Laurentian areas of Canada and the eastward areas of the United States, with other land stretching into the Atlantic, provided at least part of the sediment. It is pointed out that the deposition of sediment, leaving out of account matters in solution, takes place over a smaller area than that of the land from which it is derived. This would seem to indicate that though general conditions of stability of land-areas obtained, fluctuations of distribution occurred throughout the history of these periods in North America. Land to the westward, an extension of the Asiatic continent eastward, may very likely have sent important contributions towards the making of the future land. But, as I have said, these are speculations which, if we had all the complicated facts before us instead of buried beneath the sea, might not yield satisfactorily to imaginative reconstruction, much less to minds in the position that ours are now.

It is highly probable that many fluctuations of level occurred, and that sediments were raised above the level of the sea and again redistributed. Still, on the whole the locus was the same, and so in time the sea-bed got built up, until the internal forces of the earth, reacting upon this varied sedimentary load, began their rôle of mountain- and land-building, and the establishment of the western area of the North American continent.

In like manner, we might trace the growth of the European continent as it now exists, together with that of a large part of Asia, where the record of the rocks tells the tale of long-continued periods of sedimentation, culminating in the upheaval of the Alps, the Caucasians, the ranges of Turco-Persia, and the Himalayas.

The story might be extended to South America and Australasia, but for the purpose of illustration the two examples given are sufficient.

The later rocks of the series more or less represent land condi-

tions consisting of lacustrine and fluviatile deposits, showing that the establishment of these land-areas dates from a very early time.

The history of these Cretaceous and Tertiary areas is one of both maturity and decay. They are in some portion of the latter stage now, and eventually will probably give place to new land-areas formed out of rocks now being constructed in the sea-beds from their own dissolution.

Lithologic Characteristics and Differences of the Rock-groups of some of the Great Periods.

It is a remarkable fact, but no less true, that there are not only palæontological homologies running through the rocks of the Tertiary group over the known world, but lithological analogies also. There are diversities truly, but similarities also. Though conditions are recurrent through the ages, as pointed out so strongly by the late Sir Andrew Ramsay, it is recurrence with a difference. There has been lithological evolution as well as organic.

When we go further back in the world's history and consider the rocks constituting the Carboniferous formation, the persistence of characteristics over large areas of the earth's surface is most striking. The repetition of coals, sandstones, and shales in the Carboniferous of the North-American Continent and in that of Great Britain seems almost to point to a common origin. They repose upon great limestone formations, indicating deep-sea conditions. The coal formations in both countries indicate, according to all reliable observers, a gradual sinking of the earth's crust and simultaneous building up of the land by sediments of sand and mud, so that successive terrestrial growths and land-surfaces are marked by each bed of coal, excepting indeed in those cases that may arise from floating vegetation gradually getting water-logged and sinking onto an estuarine floor. These long-prevailing conditions are represented by thousands of feet of rocks. It is evident from the lithological resemblances, the occurrence of minerals in the same form and from similar orders of succession, that the physiographic conditions producing them were of world-wide extent and vast continuance. The character of the flora was the same in the arctic as in the temperate regions, and doubtless the maintenance of similar geographic conditions was one element in this result.

If, again, we cast our eye over the description of the Trias of Great Britain and that of the United States, the same characteristics occur in both. They are not at all like the Carboniferous, which they succeed through the Permian phase; yet, though the broad Atlantic now divides them, they indicate like physiographical conditions—strange to say, they are even of the same colour, deep red and grey, and the characteristic fossils are footprints of reptiles. Red sandstone, red and grey marls, and beds of salt

predominate. Gypsum is a characteristic mineral. There is strong evidence of inland salt lakes, lagoons, and tidal shores.

It is often said, and with great force, that it is impossible to determine the age of a rock or the formation to which it belongs by its lithology. That is true, looking at the rocks separately and individually, for the same individual conditions occur in all the ages, but notwithstanding this there are broad lithological resemblances or groupings, what is called *facies* when applied to the organic remains. There are coal beds in the Western States of North America, but no one would mistake the Laramie or Tertiary rocks of those countries for Carboniferous any more than the Oolite rocks in Scotland, in which coal is found, could be mistaken for the Coal-measures; nor yet the Lias, though ironstone is found and worked to a large extent in the Cleveland district of Yorkshire. There may be land and sea conditions and deep-sea conditions represented in the rocks of any age, but independently of the fossils there are characteristic rock-groups which distinguish the one period from the other.

Even so superficial a stratum as the Drift has common characteristics over Britain, Europe, Asia, and North America, a fact which does not merely point towards analogous climatic conditions but towards similar physiographic conditions on a large scale. Indeed some geologists think the Glacial period resulted from the physiographic conditions which then obtained. Then again, to come towards historic time, the post-Glacial deposits that fringe our coasts, the estuarine clays and forest-beds have a striking resemblance over very large areas, and even continents. Indeed, so important seemed these characteristic similarities and differences to our predecessors in the science of Geology, that they constructed, wiped out, and reconstructed the earth's surface and crust again and again by the aid of cataclysmic convulsions. The influence of Lyell was all towards showing the fallacy of these inferences, but then the pendulum swung too much the other way, and we were led to dwell inordinately on continuity and recurrence.

The uniformitarian theory was a great boon in bringing men's minds down from the region of pure imagination to that of fact. It was a good working hypothesis, but, like most generalisations, was a little one-sided. It represented too exclusively one side of the phenomena of the earth's history to the neglect of another—that of development. I am not one of those who think the doctrine of uniformity a "fetish," but rather a theory that kept us within the range of practical fact. For my part, I am prepared to accept anything that can be *proved*, but until the proof is forthcoming we are on safe ground if we confine ourselves to agencies of which we have experience.

T. MELLARD READE.

(*To be continued.*)

SOME NEW BOOKS.

DISCOVERY OF LAKES RUDOLF AND STEFANIE, a narrative of Count Samuel Teleki's exploring and hunting expedition in Eastern Equatorial Africa in 1887 and 1888. By his companion Lieut. Ludwig von Höhnel. Translated by Nancy Bell (N. d'Anvers). 2 vols. London: Longmans, 1894. Price 42s.

THE lately issued English translation of Ritter v. Höhnel's narrative of the discovery of Lakes Rudolf and Stefanie has brought this most interesting expedition before the notice of the British public, and gives us an opportunity of taking stock of the principal results arrived at. It will be obvious to any one who reads the volume, or who knows anything of the history of the business, that Count Teleki's main object in starting it was "sport"—that is, the killing and slaying of the larger mammals. The hunting passion is still so prevalent, even among the most highly-civilised races of mankind, that parties of American, German, Austrian, Italian, and, above all, British sportsmen are every year engaged in hewing their way into the still untraversed interior of the Ethiopian Continent in order to shoot the elephant, the giraffe, the rhinoceros, and specimens of the hundred species of antelopes by which it is so abundantly tenanted. To give some idea of the exuberance of mammal life in Africa as soon as the ordinary tracks are left, we have only to turn to the writings of any of the latest explorers. Vice-Consul Sharpe, speaking of a recent journey to Lake Mweru (*Geogr. Journ.*, vol. i., p. 524) says, "I doubt if game can, anywhere in Central Africa, be more plentiful than in the Mweru and Luapula countries, though there may be districts which have a greater variety. *Cobus vardoni* and *C. lechee* run in enormous herds. Buffaloes and zebras are also there in vast quantities." Captain Swayne gives a similar account of the abundance of larger animals in the interior of Somali-land, which he has lately visited.

In the present instance, however, unlike some of his brother shooters, Count Teleki was not wholly devoted to sport. He had the good sense to take with him Ludwig, Ritter von Höhnel, a lieutenant in the Austrian navy, whose acknowledged abilities have converted what was planned primarily as a sporting excursion into a scientific expedition of first-rate importance, it having resulted in the discovery of two new large African lakes previously only known in Europe from vague rumours.

Count Teleki and Lieutenant von Höhnel started from Zanzibar with a full equipment, including, among other articles, a large canvas boat, in February, 1887. Landing at Pangani, in German territory, they made the best of their way to Taveta, the "hunter's paradise" as it has been termed, at the foot of Mount Kilima-njaro. Hence, a slight *détour* to Kilima-njaro and the adjoining smaller volcanic crater of Meru was successfully carried out. The summit of Kilima-njaro was not quite attained, but Count Teleki reached a (calculated) height of 17,387 feet, and a collection of the highest-growing plants was made.

On July 15 the expedition finally left Taveta on its way north, passing round the eastern slopes of Kilima-njaro, and thence taking nearly the same route through Masai-land as that traversed by Mr. Joseph Thomson in the Royal Geographical Society's East African Expedition of 1883-4. In Kikuyu-land, where much difficulty was experienced with the natives, the party diverged to the right, and in the beginning of October reached Ndoro, at the eastern base of Mount Kenia, where they were glad to have a short respite from their laborious journey. The tents here were pitched facing Mount Kenia, so that the travellers might look at its snowy summit whenever the canopy of clouds which generally shrouds it from mortal eyes was lifted. The western side of Mount Kenia is so steep that snow can only remain on it here and there, and it is therefore termed by the Masai the "Oldonyo egeré," or "spotted mountain." From this station Count Teleki attempted an ascent of Mount Kenia. This mountain is surrounded by a belt of dense forest, through which the explorer and his men had to cut their way. At a height of about 10,100 feet the bamboo thicket was left behind; 500 feet higher the trees became much less numerous, and beyond that height only a few isolated specimens occurred. As on Kilima-njaro so on Mount Kenia, the characteristic tree is a gigantic Groundsel, allied to, but not identical with, *Senecio Johnstoni*, discovered by Mr. H. H. Johnston on the former mountain.[1] When, however, the English translator tells us that a "humming-bird was found on Kilima-njaro at a height of 13,100 feet," we are compelled to refer to the German original for an explanation, and find, as we had anticipated, that the bird in question was not a Humming-bird but a Sun-bird (*Nectarinia*), and probably *N. johnstoni*, a specimen of which has lately been brought back from Mount Kenia by Dr. J. W. Gregory. At the height of about 15,000 feet Count Teleki had a good view of the crater of Kenia, which he calculated to be from 1,000 to 1,200 feet in circumference. The bottom of the crater lay some 600 feet lower than the rim, covered with snow and ice. Count Teleki made the excursion to Kenia alone, his companion being left at Ndoro to recover his health, which had suffered much from the journey.

On November 4, after a month's stay at Ndoro, v. Höhnel's health was re-established, and he left to join Count Teleki, who had advanced a little for shooting purposes. Proceeding north-west over the spurs of the Aberdare Range, which lay to their left, at an elevation of about 7,000 feet above the sea-level, the united party crossed the Equator on November 9, and found themselves in a district inhabited by numerous bands of Masai, with whom, however, they managed to keep on good terms. Proceeding a little further, it was determined that Count Teleki should go direct to Lake Baringo with the main body of the caravan, while v. Höhnel should make a diversion eastwards, through Leikipia, as this high steppe is called, and follow up the river Guaso Nyiro, which flows to the north after rising from Mount Kenia and the Aberdare Range. Von Höhnel pursued the course of this hitherto unexplored stream for several days, when he was compelled by bad health to turn back and rejoin his companion at what Mr. Joseph Thomson calls "the pleasant resting place" of Nyemps situated near the south end of Lake Baringo. Here a prolonged stay of two months was made, during which period various

[1] Mr. E. G. Baker kindly informs me that the Senecio of Kenia is new, and will shortly be published as *S. Keniensis*. There are, therefore, now known four of these curious arborescent groundsels of the African mountains—namely, *S gigas*, from Abyssinia, *S. Manni*, from the Cameroons, and the two above mentioned.

sporting excursions were carried out by the Count, while von Höhnel was again brought nearly to death's door by fever and dysentery.

We now come to the adventures detailed in the second volume of the English translation, which are of greater interest, as relating to a country previously unexplored. The new lakes lying north of Baringo had been heard of by Joseph Thomson, and are conjecturally represented in the route-map of his East African expedition. A bold push was also made towards them by Messrs. Jackson and Gedge during their journey to Uganda through Masai-land in 1889-90, subsequent to Count Teleki's expedition. But these explorers were turned back by the natives in the plains of Ngaboto, in consequence, as they believe, of the bad behaviour of some members of Count Teleki's party so that, in fact, Count Teleki and Lieutenant v. Höhnel are the only Europeans that have, as yet, visited Lakes Rudolf and Stefanie.

After their halt at Lake Baringo the party finally left for the north in the beginning of February, 1888, and soon found themselves in difficulties from want of water. They would have been also miserably short of provisions had it not been for the successful shooting by Count Teleki. Quantities of game, chiefly buffaloes and rhinoceroses, were met with on the plateau now crossed, which was bordered on the east by a new chain of mountains—named General Matthews' range, after the well-known premier of Zanzibar. After about fifty miles transit over the waterless steppe, a half-dried-up swamp was found at the base of Mount Nyiro containing "a little thick, green, slimy fluid" with which the travellers were at last able to wet their lips. Here also the first natives of the Samburu district were met with. They are a pastoral race, called Burkeneji, closely related to the Masai in genealogy and language. According to their information the two much-looked-for lakes were not far off to the north. A few days later, accordingly, the party arrived within view of the southern end of Lake Rudolf. Before them lay a district entirely covered with black streams of lava and dotted over with extinct craters, while from one conical mountain (afterwards named the Teleki volcano) ceaseless clouds of smoke arose. Almost at their last gasp, the travellers rushed on down to the water which lay before them clear as crystal, but to their bitter disappointment when they reached it found it to be saline. Nevertheless, it afforded them a refreshing bath, and upon treatment with tartaric acid was sufficiently improved to quench their thirst. Proceeding onwards from March 6 for nearly a month, the travellers pursued their route northwards along the eastern shores of Lake Rudolf through a miserable and thirsty country. The large "Crater-Island" in the lake which they passed presented an extraordinary appearance, being made up of a series of apparently extinct volcanoes, some of which rose to a height of 400 feet. Further north, fortunately, the country improved a little, and sufficient game was obtained to support the lives of the party, while the discovery of a small stream of fresh water flowing from Mount Kulall, apparently an extinct volcano of about 4,000 feet in altitude, materially improved their position. Further on, hippopotami and elephants were found, while the shore was haunted by immense numbers of lake-loving birds. At length, after fifty-four days' wandering in an all but uninhabited land, nearly bare of fresh water and vegetation, the travellers found themselves among the Reshiat, a pastoral race of Gallas that inhabit the northern shores of Lake Rudolf. Beyond them stretched a flat tract of country overgrown by impenetrable forest, from which a five-peaked mountain-mass (Nakuà) and other heights rose in the distance. On the whole, the month's stay among the Reshiat was a peaceful

one, and the travellers had every reason to be satisfied with their reception, although their plans for returning southwards on the other side of the lake were not approved of and could not ultimately be carried out. They managed, however, to effect a successful reconnaissance of Lake Stefanie, a much smaller sheet of water, about 70 miles long and from 13 to 15 broad, which lies to the east of the northern end of Lake Rudolf. Like Lake Rudolf, Lake Stefanie is brackish, and has apparently no exit; its altitude was calculated at 1,740 feet, about 440 feet above that of Lake Rudolf. There are said to be two villages belonging to one of the Galla tribes near the northern shore of Lake Stefanie, where a small stream runs into it, but the adjoining district is almost uninhabited.

After a month's stay among the Reshiat, Count Teleki and his followers turned homewards, pursuing the same track along the eastern shore of Lake Rudolf, but then diverging westwards through the districts of Turkana and Suk, to their former quarters on Lake Baringo. In their return hence to the coast we need not follow them; it will suffice to say that they reached Mombasa on the morning of October 24, 1888, after about twenty-one months' absence.

The scientific results of Count Teleki's expedition have been by no means inconsiderable. The geographical and ethnological information amassed has been worked up by v. Höhnel, and published in a special part of *Petermanns Mittheilungen*, while the Geological observations have appeared in the *Denkschriften* of the Imperial Academy of Sciences of Vienna. The Zoological and Botanical collections have been entrusted to various specialists, and memoirs upon them have appeared in the *Sitzungsberichte* of the same Academy. More than a thousand specimens of Coleoptera were collected by v. Höhnel during the journey, and are referred to 247 species, among which nearly one-fourth are found to be new to science. Dr. Schweinfurth, of Berlin, a well-known authority on African Botany, undertook the flowering plants, among which we see registered *Senecio serra*, "the most elevated flowering-plant on Kilima-njaro, growing at a height of 16,278 feet, between volcanic ashes and rocks"; a curious new *Lobelia* (*L. Telekii*) from the western slope of Kenia, and a new form of Compositæ (*Höhnelia*) from Ndoro.

Of the mammals met with during the expedition, no account seems to be given except Count Teleki's "game list," and certain incidental remarks in the narrative, upon some of which a few comments may be added. The supposed new rhinoceros found on Lake Rudolf is not very clearly described. Its chief peculiarity is its "small head" and diminutive size, being one-third less than that of the ordinary *R. bicornis*. On the other hand, the horns are "finer and more pointed" and much more flattened. The dimensions of the horns are not given, nor is it stated whether specimens of them were brought home. The occurrence of Grévy's Zebra (*Equus grevyi*) on the shores of Lake Rudolf is a fact of much interest, as it shows that the fauna of the Shoa Highlands descends nearly to this district. Originally described from a living animal sent to Paris by King Menelek, this zebra has recently been obtained by Captain Swayne and other explorers in the interior of Somali-land. But Ritter von Höhnel might as well have given us an original drawing of this zebra, instead of copying (without acknowledgment) the figure of the flat skin published in 1890 in the *Proceedings* of the Zoological Society of London. The "unknown Gazelle" of which the horns are figured (vol. i., p. 256, in the English translation) would appear to be Grant's

Gazelle (*Gazella granti*), which is mentioned by name in other passages of the narrative. The so-called "Kaama" antelope met with in the Baringo district (vol. ii., p. 11), is, no doubt, *Bubalis jacksoni*, lately described by Mr. Oldfield Thomas. The occurrence of Waller's Gazelle (*Lithocranius walleri*) in the vicinity of Lake Rudolf is likewise of much interest, especially the observation that it "so much resembles the giraffe in its long neck and sloping haunches" that the explorers named it the "Giraffe-antelope."

On the whole, the English translation of v. Höhnel's volume is fair enough and reads well. The paper, the printing, and the impressions of the numerous illustrations are certainly superior to those of the German original. But its exactness can by no means be relied upon, and some ridiculous blunders are made as regards the scientific terms. We are, for instance, introduced to "humming-birds" and "tigers" in Africa, the horns of an antelope are called "antlers," and we are told that *Hydnora* belongs to the "Cytinaceæ genus." The proofs of the sheets should have been submitted to someone who had an elementary knowledge of Natural History.

P. L. SCLATER.

MAN, THE PRIMEVAL SAVAGE: His Haunts and Relics from the Hill-tops of Bedfordshire to Blackwall. By Worthington G. Smith, F.A.I., F.L.S. With two hundred and forty-two illustrations by the author. 8vo. Pp. xvi., 350. London: Stanford, 1894. Price 10s. 6d.

MR. WORTHINGTON SMITH has done yeoman's service for the elucidation of the history of primitive man in this country, and he has now made us still further his debtors by collecting together his contributions to various journals, and filling in the details of those discoveries which he has from time to time brought briefly to our notice.

The first part of the book contains an account of the lake-side dwelling of man, with his weapons of stone, found at Caddington, near Dunstable, of which the first description was contributed by Mr. Smith to NATURAL SCIENCE for November, 1892. The latter part describes some of the relics of primitive man as found on the banks of the Lea, from its source near Dunstable, in Bedfordshire, to London; together with a description of a primeval living place, or Palæolithic floor, at Stoke Newington, London, of which, it will be remembered, the first notice appeared some years ago in *Nature*.

But the book is by no means a mere reprint; the majority of it is entirely new and rewritten; the facts remain, added to and supplemented by Mr. Smith's later discoveries and re-examinations.

By "primeval savages," Mr. Smith means Palæolithic men—the men who designed, made, and used the most ancient of all stone weapons and tools recognised as such by antiquaries and anthropologists. Our knowledge of the primeval savage and his mode of life is at present little better than a shadow. But his weapons are common and well-known, and from these weapons we may gather much that is instructive of the men who formed and used them. The author believes that "man did not live in what is now Britain before what is known as the last great Glacial period."

In his introductory chapter, Mr. Worthington Smith gives a sketch of the more important osseous remains of primitive men yet discovered, and furnishes illustrations of the Neanderthal, Canstadt, Spy, Engis, Cro-Magnon, Grenelle, Furfooz, and Truchère skulls and crania, comparing them with those of present-day

Melanesians and Chimpanzees, and showing the great physical and mental gulf between man and any living member of the anthropoid apes. Of course such material as we possess is quite insufficient to permit of any accurate deductions, and it is only by the patient and careful research of such investigators as Mr. Smith that we can hope to obtain the light necessary to partially solve this important and interesting question. After a brief and vivid, though not particularly pleasing, sketch of the habits and life of our ancestors in their wild state and savage homes, Mr. Smith plunges into the main subject of his work, Caddington.

Caddington, the site of the discovery of an undisturbed living and working place of primeval man, is thirty miles from London, and the excavations, specially made for Mr. Smith, are half a mile west of the village, in so-called brick-earth resting upon chalk-rock, about 600 feet above Ordnance datum. The nature of the deposits and the contour of the country around leads Mr. Smith to the conclusion that in former times the higher ground bordered a lake, or series of large and confluent pools of water.

Chapter IV. treats of the geology of the Caddington position, the nature of the deposits, and of the old Palæolithic floor, with the flints found thereon. Of the flakes found on this floor Mr. Smith has replaced more than 500 on to either other flakes, or more or less perfect implements, or cores, from which they were originally struck off. This proves that the remains left behind by the inhabitants of this part have suffered little or no disturbance since their time.

Chapter V. is devoted to the story of the discovery of implements in the Dunstable district, and the gradual tracking of the gravel which contained them to various pits and excavations, until at last the reward came, and the Caddington gravel pits yielded up their treasures. There are two or more types of implements described by Mr. Smith. The first, or "ochreous" type, which came from the upper brown stony clay, and which are all slightly abraded, and the white, lustrous, and sharp-edged type found on the Palæolithic floor itself. The evidence at hand goes to show that the latter are the more recent of the two sets.

It was in May, 1889, while making an examination of the exposed vertical bank of a clay pit, that Mr. Smith noticed indications of a horizontal streak in the brick-earth, at about six feet from the surface. He had previously suspected the existence of a floor, or working site, from the fact that the sharp and unabraded chips of flint were usually met with in the fallen material. In this streak, he here and there noticed a stone embedded, and at one place saw a small flint half projecting from the face of the excavation. On removing the flake, he found it to be a beautifully made scraper, and saw that it was identical with one he had found two years before in the same locality. More discoveries followed, but it was not until the end of March, 1890, that he established the fact of the finding of a Palæolithic floor or living surface, similar to the one discovered by him several years previously at Stoke Newington.

The succeeding sixty pages of the book are taken up with a description of the flint implements and flakes found on this Palæolithic floor, beautifully illustrated by Mr. Smith's own drawings, and showing the actual implements themselves, as well as the restoration of them to the rough block from which, in many cases, they were made, by the fitting on of chipped fragments found associated with them.

In Chapter X. we have a description of the Caddington position in

Palæolithic times, and in Chapter XI. a discussion as to the age of the implements themselves. Pages 179-306 deal with "Traces of primeval man," and contain a record of discoveries of implements found between Bedfordshire and London, dozens of which are figured and all of which are of Palæolithic age. The rest of Mr. Smith's work treats of Neolithic weapons which have been found from time to time over the same area, together with notes on earthworks, roads, and track-ways near Dunstable; early British hut-remains, deneholes, graves, tumuli and skeletons from Dunstable Downs. In the above imperfect sketch we have endeavoured to show the contents of this interesting book, and have not pretended to offer any critical remarks. The book is emphatically one to buy, to read, and to ponder over. From it those interested in the early history of their race will learn much, while the sketch map will enable them to follow the author in his topographical details, and assist them to obtain an excellent idea of the mode and conditions of life of their remote ancestors. Of the illustrations of flint implements we need scarcely speak, suffice it to say that they are Mr. Worthington Smith's, and that the variety and number of them should enable many a tyro to supplement our knowledge of their distribution.

We observe with pleasure, from a circular recently received, that Mr. Smith will conduct an excursion of the Geologists' Association to Caddington on May 26, in order to explain to them the structure of the district and his discoveries. May we be there to see!

Horned Reptiles in Scotland.

On Some New Reptiles from the Elgin Sandstones. By E. T. Newton. *Phil. Trans.*, vol. clxxxiv., B., pp. 431-503, pls. xxvi-xli. (1893).

For some years past, palæontologists have been tantalised with reports of the occurrence of remains of reptiles allied to the Dicynodonts of Africa and India in the Triassic sandstones of Elgin; but with the dilatoriness of most Government departments—whether it be in deciding to build ships or to describe fossils—the Geological Survey of Scotland, to whom the specimens belonged or were confided, maintained a strange reserve on the subject. After a protracted period of patient (or shall we say impatient?) suspense, those interested in the subject have, however, been rewarded with the appearance of a memoir on the remains in question, which leaves little to be desired in point of completeness or in wealth of illustration; and we here beg to offer our congratulations to the Council of the Royal Society in having displayed such a wise liberality to the author in the way of plates.

Not only does the memoir before us fully endorse the aforesaid reports as to the occurrence of Anomodont reptiles in the Elgin sandstones, but it likewise shows that these animals were represented by numerous and diversified types, all of which were, however, more or less closely allied to South African forms. Unlike the African representatives of the group, which for the most part occur in the form of well-preserved skulls and limb-bones, the Elgin specimens when placed in the hands of the author of the memoir before us were in the shape of most unpromising impressions. Fortunately, he hit upon the plan of taking gutta-percha casts from these natural moulds, with the result that, after much patient labour, he succeeded in restoring the skulls of these ancient reptiles in a manner which makes their characters fully apparent; and it is from such casts that most of the figures in the memoir have been drawn.

The first remains that the author takes in hand are certain skulls

indicating reptiles closely allied to the typical African genus *Dicynodon*, which are referred to several distinct species. He is, however, of opinion that they cannot be included in that genus, and accordingly refers them to a new one, for which the name *Gordonia* is suggested; but we have considerable misgivings whether the features he regards as of generic value are really entitled to such importance. Be this as it may, we cannot help thinking that in giving to each species of this reputed genus the name of a person, the author has paid a poor compliment to him in whose honour the genus itself is named. If a genus founded on a personal name means anything at all, it means that the animals so named are dedicated to the person whose name is thus employed. Accordingly, *Gordonia* will signify Gordon's reptiles; but if we are to have *Gordonia traquairi*, *Gordonia huxleyana*, etc., etc., the system of what we may call personal nomenclature appears, to our thinking, reduced to an absurdity. We are not fond of personal nomenclature at all, but the degree to which it is carried here is certainly excessive, seeing that, out of the nine forms described, seven have such names both generically and specifically; while an eighth has its generic name alone thus compounded.

As the so-called *Gordonia* is closely allied to *Dicynodon*, so the skull described as *Geikia* displays equally close affinities with *Lystrosaurus* (*Ptychognathus*); but if palæontologists are right in keeping, on account of the absence of tusks, *Udenodon* apart from *Dicynodon*, the author will be justified in giving a new name to the tuskless Elgin form. We wish, however, that he would not continue to employ names like *Ptychognathus*, which, on account of preoccupation, are shown to be inadmissible; and he need not have gone out of his way here, and in the case of *Pariasaurus*, to ignore the most elementary principles of classical transliteration, by an adherence to an incorrect way of spelling which has been put right in some of the very works he quotes.

Perhaps the most interesting of all the forms described is the remarkable horned skull designated *Elginia*, two of the figures of which are here reproduced on a greatly reduced scale. After describing the characters of this extraordinary skull, which, it will be observed, has the whole of the temporal region completely roofed over, the author proceeds to discuss its affinities. Here he, unfortunately, displays such a total want of grasp of the relationships of the various recent groups of reptiles as cannot but detract from the value of his deductions. He says, for instance, that "although the Elgin Reptile undoubtedly possesses several important Lacertilian characters, and apparently finds its nearest living allies in that group, yet it presents peculiarities of structure which prevents its being referred to the Lacertilia." Seeing that it has a fixed quadrate, a lower temporal arcade, and pterygoids meeting in the middle line, we perfectly agree with the latter part of this sentence. Imagine, however, our astonishment when, after reading a few lines further, we found that the author actually includes the living New Zealand *Sphenodon* among Lacertilia, although he has separated it from the true Lacertilia. This is about as good as if some new shining light in zoology were to propose to class tigers and elephants in the same order!

When an author appears so incapable as this of grasping the relative importance of structural features in recent reptiles, it is not much good attempting to follow him in his discussions on fossils. He considers, however, that *Elginia* has its nearest ally in the gigantic *Pariasaurus* of the South African Karoo beds; and in this we believe he is right. When, however, he proceeds to observe that both

have little or no affinity with the Anomodonts, and show Lacertilian resemblances, we are again so staggered that, did we not recall the circumstance that he includes *Sphenodon* among Lizards, we can hardly realise that he intends to be serious. Has the author, we may ask, ever compared the pelvis and humerus of *Pariasaurus* with those of a typical Anomodont? and if so, does he still hold the opinion he expresses? In the same section we find a great deal of discussion as to which apertures are the posterior nares in the skulls of the African and Elgin forms, and much importance is attached to the conclusion reached. We would, however, remind the author that the Chelonia and Crocodilia show that the position of these apertures is of extremely little classificatory import. Even if he be right in stating that they have the same position as in *Sphenodon*, this in no way

Superior and right lateral aspects of cranium of *Elginia mirabilis*, together with a side view of the anterior teeth. About one-quarter natural size.

shows that the forms under discussion are more nearly related to Lizards than to Anomodonts; since we thought every tyro knew the structural resemblances between the tuatera and the last-named group.

Although we regret we cannot congratulate the author on the manner in which he has handled an admittedly difficult subject, yet science is indebted to him for having so carefully restored the skulls of the Elgin Anomodonts, and for having provided us with figures which admit of our drawing our own conclusions.

We may add that the close similarity of the Elgin fauna to that of the African Karoo beds, and the Indian Panchet and Maleri beds, serves to confirm the original view as to the Triassic age of both the latter.

R. L.

The Australian Mud-Fish.

TRAVELS FOR ZOOLOGICAL RESEARCH IN AUSTRALIA AND THE MALAY ARCHIPELAGO. [Zoologische Forschungsreisen in Australien und dem malayischen Archipel.] Pts. I. II. Edited by Richard Semon. Jena: Gustav Fischer, 1893.

THE munificence of Dr. Paul von Ritter, founder of the Professorship of Phylogeny and Theories of Descent at Jena, has already rendered signal service to Biological Science. We have often referred to the researches of the Ritter Professor, Dr. W. Kükenthal, which are of fundamental importance for the understanding of the vertebrate animals. We now have to welcome the first instalment of another great work rendered possible by the same generous patron. Between June, 1891, and the end of the following year, Professor Richard Semon, of the University of Jena, was enabled to undertake one of the most important scientific expeditions for collecting that has hitherto been made, to Australia and the Malay Archipelago. Now, the collections have been assorted and arranged, and the publication of the results is commenced in a series of fine quarto volumes. Dr. Semon estimates that there will be five volumes devoted to the Vertebrata, the first relating to the *Ceratodus*, the second and third to the Monotremes and Marsupials, the fourth and fifth to Sirenians, Edentates, *Amphioxus*, and miscellaneous other types.

Professor Haeckel appropriately prefaces the work with some general observations on the relationship of the Australian Fauna, and then Dr. Semon contributes a brief sketch of his journey, enumerating the various centres at which collections were made. Biologists, however, will turn at once with greatest interest to the first part of Dr. Semon's detailed monograph of *Ceratodus*, which is destined to inaugurate a new era in our knowledge of the recent mud-fishes. As patriotic Britons we peruse it with mingled feelings; for had not the collector despatched ten years ago by the Royal Society of London and the University of Cambridge betrayed his trust, the credit of unravelling these problems would have belonged to our own nation.

It is impossible to do justice to Dr. Semon's work on the Australian mud-fish in a brief notice; and it is too early to discuss the conclusions, only the first stages in the life-history of the animal being as yet described in a general way. The facts in reference to the distribution, mode of life, and propagation of *Ceratodus* are set forth in a more exhaustive manner than by previous observers; and the embryological part is illustrated by exquisite plates of segmenting eggs and of immature fishes. *Ceratodus* is believed to be confined to the Burnett and Mary rivers, Queensland; and although, as already generally known, it crops the vegetation with its great teeth, the most important part of its food consists in the small animals living between the fronds and leaves. It seems to be quite powerless to move on land, and it never lies dormant in "cocoons." The eggs are laid separately and loosely among the vegetation; the fore-limbs begin to appear at the end of about fourteen days, and the hind-limbs not before two-and-a-half months. In the course of development, *Ceratodus* presents most resemblance to the Cyclostomes and the Amphibia; but it is noteworthy that external gills are absent at all stages, and there is no trace of a suctorial mouth. There is no rudiment of a connection between the pectoral and pelvic fins; and many features are entirely peculiar. The large dental plates result from the fusions of small teeth, exactly as might have been expected from what is known of extinct mud-fishes. As to the all-important

question of the mode of development of the limbs, we must be content to wait for the details to be published later; we can only hope that the delay in issuing the remaining sections of the volume will not be long.

Geography as it is Taught.

Philip's Systematic Atlas, Physical and Political, specially designed for the use of Higher Schools and Private Students. Containing over 250 maps and diagrams, in 52 plates, with an introduction and index of 12,000 names. By E. G. Ravenstein, F.R.G.S. London: George Philip & Son, 1894.

This astonishing production is, say Messrs. J. Scott Keltie, H. J. Mackinder, and E. G. Ravenstein, in a preface, "intended to meet the requirements of pupils in higher schools, of teachers and of other students of geography, for whom neither the ordinary school atlas nor the general reference atlas is entirely adequate." If, then, this atlas represents in any way the methods of teaching favoured by these gentlemen, we are sincerely sorry for any students who come under the hands of Messrs. J. Scott Keltie, H. J. Mackinder, and E. G. Ravenstein. Geography, as certainly Mr. Scott Keltie and Mr. Ravenstein ought to know, is much too important and interesting a science to be distorted and obscured by the wholesale introduction of imperfect text-book information from other sciences. There are, for instance, two plates supposed to illustrate the geographical distribution of plants and of animals. On one plate there are eleven separate maps, on the other nineteen, most including at least a hemisphere and illustrating such points as the range of rhubarb, the Cassowary tree, "nut" trees, Wallace's zoogeographical regions, serpents and lizards, the beaver, and the orang-utang, the orders of mammals, and so forth. It is needless to say that it is impossible to give correct information on such a scale, and that the maps as they appear are useless to the zoologist and botanist and pernicious to the general student. There is, again, a "geological" map of Europe on the scale of 637 miles to the inch, coloured to show "Cainozoic and Recent," "Mesozoic," "Palæozoic," and "Igneous" formations. Two little hemispheres of about three inches diameter show the religions of mankind under the simple if unsatisfactory headings "Heathen, Hindu, Bhuddist (*sic*), Mohammedans, Christians." Two similar maps show the races of mankind—"Aryans, Semites, Hamites, Mongolians, Malays, Hottentots, Negroes, Australians and Papuas, Eskimo, American Indians, Mixed." Do Messrs. Scott Keltie, Mackinder, and Ravenstein regard these tit-bits of inaccurate folly as geography? They certainly are neither geology nor anthropology.

This desire to crowd in a senseless appearance of information reaches even the topographical plates. Thus in a corner of the plate entitled Great Britain and Ireland is a "map" of London on the scale of three miles to the inch, with the following eight references:—1. Zoological Gardens; 2. Kensington Museum; 3. National Gallery; 4. Westminster Abbey; 5. British Museum; 6. Law Courts; 7. St. Paul's Cathedral; 8. The Tower. For whose benefit is this, and why the invidious exclusion of the Houses of Parliament and Madame Tussaud's?

Apart from the singularly incompetent choice of subjects and editing, the plates are clearly printed and, at least by daylight, well coloured. The physical maps are specially useful; the political maps are up to date; the maps showing densities of population are of a kind to please those people who like to think they know

about things. The commercial maps are not so satisfactory; it is difficult to follow the highways of commerce by land and by sea upon them. The editors say that, in the construction of the maps showing the distribution of plants and animals, "particular attention has been paid to such products as are useful to man and enter largely into commerce." We have already referred to the absurd inadequacy of these, but in the introduction, as distinguished from the preface, the contradictory statement is made that "our maps show the supposed native countries of these plants" (*i.e.*, characteristic plants and those of economic importance), "and not the countries in which they are being cultivated at the present time." Messrs. Scott Keltie, Mackinder, and Ravenstein have associated their names with what is in many places a mere burlesque of the great science of geography.

Dredging and Distribution in Danish Seas.

DET VIDENSKABELIGE UDBYTTE AF KANONBAADEN "HAUCHS" TVOGTER I DE DANSKE HAVE INDENFOR SKAGEN I AARENE, 1883–86 (Chef: Premierlieutenant C. F. Drechsel). Udgivet paa Bekostning af Ministeriet for Kirke- og Undervisningsvæsenet ved C. G. Johan Petersen. Pp. 464, 6 pls. and 2 maps, 4to; atlas of 43 maps, fol. Kjöbenhaven, 1889–93.

THIS work, which has just been published under the auspices of the Danish Government, is one of no common interest for those who believe that a careful investigation of the conditions affecting the distribution of common animals within a limited area is likely to be of as much utility in the solution of important biological problems as is the collection of new species from far-off lands.

In the year 1883, Captain Drechsel applied to the authorities of the Zoological Museum in Copenhagen to recommend him a naturalist who would spend a few weeks on board his ship making collections for the Museum and investigating the animal life of the Danish seas in general. The choice fell upon Dr. Petersen, and has been amply justified by the results of his work published in the volume now under notice.

During the first season Captain Drechsel and Dr. Petersen elaborated a plan of campaign, which was "not merely to seek for species new to the fauna of the district, but first and foremost to acquire a knowledge of the distribution of the commonest animals, and at the same time to investigate as far as possible those natural conditions, such as temperature, composition of the bottom, etc., which might be supposed to exercise an influence upon the distribution and life of the animals. This plan was carried out in the course of the three following summers, 1884-86, and, with this object in view, dredgings were made at as many stations as possible in the Kattegat, so that on the basis of these observations conclusions might be drawn regarding the distribution in those waters of at all events the commonest animals, especially the invertebrata." Wherever possible both dredge and trawl were used, and the process was carried on till it became possible to predict with a fair amount of certainty what species would be obtained from any particular haul.

More than 500 stations were occupied, and in almost every case, in addition to the exact geographical position, there are given the depth, the nature of the bottom, the temperature of the water and its specific gravity both at the surface and the bottom. The material obtained was placed in the hands of different specialists for examination, and their reports are incorporated in the present volume. The

five fascicules of text include memoirs by different specialists—on the Echinodermata and Mollusca, by Dr. Petersen himself; on the Cephalopoda, by Dr. H. J. Posselt; on the Polyzoa, Annulata, Cœlenterata and Sponges, by Inspector Levinsen; on the Crustacea Malacostraca, by Dr. Meinert; on the Diatoms, by Professor Cleve; and on the Bottom Deposits and the Hydrography of the Kattegat, by Dr. Rördam.

Turning first to the chemical and physical portions of the work, we notice that the greatest percentage of calcium carbonate is found in the deepest part of the Kattegat, perhaps in correlation with the saltness of the water, while the deposits from the bottom of the Baltic contain little, if any, of this substance. It appeared that during the three years (1884–86) in which the observations were made, the saltness of the water in the Kattegat increased, while at the same time the mean temperature diminished. This result is confirmed by independent observations from other sources, and is attributed to an unusual prevalence of N. and N.W. winds during those seasons. These facts are shown very clearly by a plate containing nine little maps.

The account of the Echinodermata and Mollusca by Dr. Petersen need not be noticed at length here, except for the purpose of calling attention to the admirable series of 26 maps by which the distribution of the various species is illustrated. No new forms are here described, though some additions to the Danish fauna are recorded. The Mollusca, in particular, have already been discussed by the same author in an elaborate thesis published about five years ago.[1] The section dealing with the Cephalopoda by Dr. Posselt is not merely an account of the few taken by the "Hauch," but is virtually a systematic monograph of the Danish forms, and contains some novel information of great importance. Dr. Meinert's report on the Malacostracous Crustacea is a very important piece of work, being in fact a revision of the Danish fauna, so far as this group of animals is concerned. The number of species recorded is increased from 177 to 253, and nine new species are described, of which three are referred to new genera. The memoir is illustrated by fourteen maps and two copper plates. Inspector Levinsen has dealt with the Porifera, Polyzoa, Hydroida, and Anthozoa, and in regard to the first three of these groups has given us a complete account of the Danish forms, with a very convenient dichotomous key for their identification.

The whole work is completed by a discussion of the general results by Dr. Petersen, which he has considerately printed both in Danish and English; this is naturally the most important section of all and some account of its more salient features must be attempted. The bottom of the sea is covered with three kinds of deposits, stones, sand or gravel, and clay or ooze, the last of which occurs only in the deepest places and is the most unfavourable for animal life. As to vegetation, about half the Kattegat and nearly all the Skagerack are destitute of it, and, next to the inshore waters, the richest places are the protected situations in the Kattegat. The salinity, too, increases with the depth, though it varies greatly with the seasons, the melting ice in the Baltic being, perhaps, the most important factor in this connection. As might have been expected, the temperature is most variable in the inshore waters, where it reaches 19–23° C., and least variable in the Skagerack, the deep eastern Kattegat being intermediate in this respect.

[1] Petersen, "Om de skalbærende Molluskers Udbredningsforhold i de danske Have indenfor Skagen," 8vo., pp. viii., 162, map, Kjöbenhaven: 1888.

The temperature of the bottom mud seems to be somewhat higher than that of the water in winter; for instance, while the water was about 0° C., the temperature 3–4 feet down in the mud was + 7° C. This is doubtless the reason why many animals (Gobiidæ *Carcinus*, *Cerithium*, etc.) bury themselves in winter. The influence of the physical conditions on the distribution of various animals is then discussed, and fourteen examples are given in some detail, special reference being made to different species of the same genus which appear to represent each other in different parts of the area under consideration. The author claims to have shown "that each species is distributed according to certain rules," it is seldom, however, "that one single factor is decisive, but the distribution is the result of groups of concurrent circumstances." One point of interest is brought prominently forward—"no species of animal is found everywhere in our seas." When an animal is once recognised as common it is usual in faunistic works to describe it as found "in all our sounds and fjords" as occurring "everywhere along the Danish shores," so that frequently the more abundant a species the less information is available as to its actual distribution. Dr. Petersen's careful maps furnish a marked contrast to this mode of work. After a short chapter on the distribution of certain animals outside the Danish waters, we have some observations on distribution in the post-Glacial period, from which it appears that the Baltic and Kattegat may be compared to a Norwegian fjord in which the cold water forms are not yet shut in, but in which they and the warm water forms move in and out according to circumstances. As representatives of the "cold fauna" we have *Astarte borealis*, *Idotea entomon*, *Halicryptus spinulosus*, etc., of the "warm fauna" *Ostrea edulis*, *Tapes pullastra*, and *Pecten varius*. This concludes a most important and interesting contribution to the natural history of the Kattegat—"the greatest battlefield on the coast of Europe between the ocean water and the fresh water."

WM. E. HOYLE.

THE British Oniscidæ or "Woodlice"—a group of animals somewhat neglected by naturalists—are described by Dr. R. F. Scharff in the *Irish Naturalist* for January and February, all our native species being well figured on a single plate. Fourteen species are common to Great Britain and Ireland, two occur in the large island which are not found in the smaller, while three species have been found in Ireland which have not yet been observed in England or Scotland. There are not many groups in which Ireland can claim a richer fauna than Great Britain. Foreign species receive attention at the hands of M. Adrien Dollfus, who continues his researches on the Isopoda and their geographical distribution. Four of his papers have recently come to hand, and furnish us with information on the woodlice of Venezuela, the Pyrenees, and the Seychelles.

THE Termite Ants are so constantly being brought forward in discussions on Natural Selection (see Mr. Platt Ball's paper, NATURAL SCIENCE, Feb., 1894, and Mr. Cunningham's in the present number), that those interested in the problem will be glad to know of an exhaustive paper on the constitution and development of the Termite colony, with notes on their habits, which is now begun by Drs. B. Grassi and A. Sandias in *Atti della Accademia Gioenia in Catania* (ser. 4, vol. vi.).

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments have recently been made in the Botanical world:—Mr. R. J. Harvey Gibson to the Professorship of Botany at University College, Liverpool, the endowment of which has recently been completed by a donation from Mr. Holbrook Gaskell; Dr. Carlo Casali to be assistant in Botany at the University of Rome; Dr. C. Avetta to be Professor in the University of Padua, and Director of the Botanic Gardens; Mr. J. H. Burrage, of New College, Oxford, to be Demonstrator to Professor Balfour at Edinburgh; Dr. Richard Otto, recently assistant in the Institute of Plant Physiology at the Royal Agricultural School in Berlin, to be teacher of Chemistry at the Royal Pomological Institute at Proskau o/S, while Dr. Fr. Krüger of Geisenheim takes his place at Berlin. We also note that Dr. V. Schiffner will temporarily take the place of Dr. Hallier as an assistant in the Botanic Garden at Buitenzorg in Java while Dr. Hallier makes a visit to Borneo; that Professor Julius Klein has gone to Naples to undertake some botanical studies at the Zoological Station; that Dr. Robert Regel, who has for the last few years been a junior curator in the Herbarium of the Royal Botanic Gardens at St. Petersburg, has resigned this position to become Privatdocent in the University, where he takes classes in the relation of Botany to Horticulture; that Mr. Alboff, who, at the cost of the Boissier Herbarium, has for the last six months been investigating the mountain flora of the Caucasus, has returned with rich spoils; and that Dr. L. Guignard has been elected President of the Botanical Society of France for 1894. We are glad to learn from the *Kew Bulletin* that the Government of Queensland has reconsidered its decision, and that Mr. F. M. Bailey, the abolition of whose post was recently reported, has been re-instated as Colonial Botanist. It would be poor economy to do away with so important a post, especially when it has been filled so long and ably by a man like Mr. Bailey, who has added largely to our knowledge of the plant products of the colony.

FURTHER good news for Botanists is that the botanical explorer, Captain John Donnell Smith of Baltimore, Md., has opened his private herbarium and library to the students of botany of the Johns Hopkins University. This herbarium is one of the largest and best selected private herbaria in existence, containing not only collections from all parts of the world, but also the plants collected by Mr. Smith and his correspondents in Guatemala, many of which are the type specimens of new species. The owner intends to present both herbarium and library to the University, so soon as a building shall be offered for their reception, and provision made for their maintenance. The library contains about 1,300 volumes, chiefly relating to American botany. Captain Donnell Smith has recently started on another visit to Central America.

MEANWHILE Botanists nearer home are likewise busy, as the following items show. Dr. Baroni of Florence intends to monograph the genus *Atriplex*, and requests materials from his colleagues. The tenth Congress organised by the French Nationa Society of Horticulture will meet at Paris during the General Horticultural Exposition, May 23–28. Among others, the following subjects will be discussed:—Chloro-

phyll in its relation to the vigour of cultivated plants; capillarity and the preparation of the soil; and means for hastening the nitrification of nitrogenous substances and rendering them more easy of assimilation. Under the direction of Professor P. Lachmann a botanical garden has been established in the mountains near Grenoble, at a height of 1,875 metres. Nor should we forget the Royal Botanic Society of London, on whose pecuniary difficulties we commented in our last number (p. 233). We then suggested that they were in part due to the Society's exclusiveness as regarded its Gardens in Regent's Park; but an important step has since been taken by the Council. A regulation has been made which will doubtless increase the popularity of the Gardens. Permission is now given to smoke therein.

AMONG Zoologists the following appointments are to be chronicled:—Dr. F. Zschokke to the Professorship of Zoology at Basle University, in place of Dr. L. Rütimeyer, who has long been in failing health; Dr. K. Zelinka to be Professor-extraordinarius of Zoology at the University of Vienna; Mr. S. H. Reynolds to be Demonstrator of Biology in University College, Bristol; and Dr. Harrison Allen to be Director of the Wistar Institute of Anatomy at the University at Pennsylvania. Professor W. Krause of Göttingen has moved to Berlin (Brückenallee, 31), where he has been placed in charge of the Anatomical Institute of the University. Mr. J. Willis Clark succeeds Professor M. Foster as Rede Lecturer at Cambridge University. We also note that Mr. C. R. Beazley, Fellow of Merton College, has been elected to the Geographical Studentship at Oxford University for the present year; and finally that Dr. Hans Bruno Geinitz, the veteran geologist and palæontologist of Dresden, now in his eightieth year, retires this month from his professorship in the Technical High School, where he will be succeeded by Professor E. Kalkowsky, of Jena.

SEVERAL scientific men have of late received honours from various universities. Cambridge University has conferred the degree of Doctor of Science on Dr. Santiago Ramon y Cajal, Professor of Histology and Pathological Anatomy in the University of Madrid, who, on March 8, delivered the Croonian lecture before the Royal Society, his subject being the minute structure of the nervous system, of which he has done so much to advance our knowledge. On the same day the degree of LL.D. *honoris causa* was conferred by Cambridge University on the Right Hon. the Earl of Kintore, Governor of South Australia, who, it will be remembered, recently travelled from Port Darwin to Adelaide in company with Dr. E. C. Stirling. The Senate of Aberdeen University has conferred the honorary degree of LL.D. on Mr. Henry Ogg Forbes, the well-known explorer, now Director of Museums in Liverpool.

THE following prizes and scholarships are open to competition. The Italian Geological Society offer a prize of 1,800 francs for the best memoir on the present knowledge of the Palæozoic and Mesozoic rocks in Italy. The memoir must be presented by the end of March, 1896. The Royal Society of New South Wales offers its medal and £25 for the best original communication (provided it be of sufficient merit) upon each of the following subjects, to be sent in not later than May 1, 1895,—on the silver ore deposits of New South Wales; on the physiological action of the poison of any Australian snake, spider or tick; on the chemistry of the Australian gums and resins. And the following to be sent in not later than May 1, 1896,—on the origin of multiple hydatids in man; on the occurrence of precious stones in New South Wales, with a description of the deposits in which they are found; on the effect of the Australian climate on the physical development of the Australian-born population. Competitors are requested to write upon foolscap paper, on one side only. A motto must be used instead of the writer's name, which latter must be enclosed in a sealed envelope; all communications to be addressed to the Hon. Secretaries, T. W. E. David and J. H. Maiden, at the Society's House, Sydney. Cambridge University announce that the Arnold

Gerstenberg Studentship, worth about £90 a year, will be awarded next May, on the results of an examination in Psychology and Logic, beginning on May 21. Candidates must have obtained honours in one part of the Natural Sciences Tripos, and have entered into residence before April, 1888. The student elected is required to devote himself to mental or moral philosophy. Guy's Hospital announce that two open scholarships in science, of the value of £150 and £60, will be examined for in September next; particulars may be obtained from the Dean of the Hospital.

Among appointments to be filled up we note the following:—An Assistant-Lectureship in Agriculture at the Durham College of Science, Newcastle-on-Tyne. A practical acquaintance with agriculture is necessary, and a special knowledge of animal anatomy and physiology desirable. The stipend is £150 *per annum*, with a share of certain fees. Applications and testimonials must be sent on or before April 6 to the Secretary, Mr. H. F. Stockdale, from whom further particulars may be obtained.

For many years the idea of a Free Local Museum to illustrate the Natural History, Archæology, etc., of the district of Epping Forest has been advocated by those interested in the subject, and especially by the members of the Essex Field Club. The Epping Forest Committee of the Corporation of London having carefully preserved the old building known as Queen Elizabeth's Lodge at Chingford, they will be approached with a view to the housing and arranging of the proposed collection in the Banqueting Hall of the Lodge. The district that the promoters propose to illustrate is, roughly speaking, that bounded by the Northern and Eastern Railway on the west, and the London and Ongar Railway on the east; and is about sixteen miles long by four wide. The Essex Field Club are willing to undertake the gathering of specimens, as well as the curatorial work necessary for the proper arrangement of the collection. A general meeting was held, in Queen Elizabeth's Lodge, on the 24th February, to consider the question. The Rev. A. F. Russell (Rector of Chingford) was in the chair, and speeches were made by Professor C. Stewart, Mr. J. E. Harting, Professor Meldola, Mr. Howard Saunders, Professor Boulger and other well-known naturalists. From a letter we have received from Mr. W. Cole, the Hon. Sec. to the Essex Field Club, we learn that the Conservators have given the Club provisional permission to place specimens in the room; but that it will be necessary to raise a small fund before the Conservators can be approached for full permission to carry out the proposed Museum. It is estimated that the sum of £300 would enable the committee to provide the necessary cases, cabinets, and museum appliances, and, considering the excellence of the idea, this money should be easily obtained. Those who desire to help the committee in furthering the educational value of this delightful forest should communicate with Mr. Cole at Buckhurst Hill.

We learn from the *Daily News* that the Geological Department of the British Museum has recently been enriched by over three hundred fine specimens of fossil plants, from the Coal-measures of Somerset, collected by the late Jas. M'Murtrie of Radstock. The same institution has been presented by the Governors of Cheltenham Hospital with the skeleton that belonged to a young Chimpanzee (*Troglodytes niger*), from Angola, which was dissected by Dr Edward Tyson and described in his work "*Orang-outang, sive Homo Sylvestris*; or the Anatomy of a Pigmie compared with that of a *Monkey*, an *Ape* and a *Man*." This, the first account of any man-like ape that could pretend to scientific accuracy or completeness, was published by the Royal Society in 1699.

The town of Macclesfield has accepted the offer of Miss Marian Brocklehurst to build and endow a Museum in the Public Park. The basis of the collection will

be a large series of Egyptian antiquities obtained by the donor herself while travelling in the East; but it is intended also to add a general collection of Natural History, with special reference to the fauna, flora, and geology of East Cheshire. Miss Brocklehurst proposes, in association with her brother, to arrange for an endowment of £100 per annum, half to be devoted to maintenance, half to the Park Keeper for curating. As the munificent donor requests to be favoured with suggestions, we would venture to remark, from our knowledge of the culture of the average Town Councillor, that no institution of the character she proposes to found can flourish under the direction of a Corporation Committee without the guidance of some educated and experienced curator. Unless adequate provision be made for such an officer, the Museum will become a mere storehouse of "curiosities" of no value to anyone.

WE have received the Seventh Annual Report of the Liverpool Marine Biology Committee and their Biological Station at Port Erin, Isle of Man, drawn up by Professor W. A. Herdman. The work of the Committee was transferred to Port Erin from Puffin Island in 1892, and the present report shows how it has benefited by the change. Two dozen workers (not 60, as the report says) have stayed at the station for varying periods during the year. The Curator for most of this period was Mr. J. H. Vanstone, who resigned in September; a new Curator will be appointed by the committee after Easter. A separate aquarium and tank-house have been built for the storage of living marine animals and plants, for facilitating observations on habits and life histories, and for exhibition to the public. During the latter part of the summer a charge of 3d. was made for admission to the aquarium, and this in four weeks produced £1 17s. 10d., from which we infer that one visitor was let in cheap or else did not get his proper change. To make the aquarium more permanently attractive than the come and go of living material renders possible, it is intended to display round the walls a large collection of Manx Invertebrata presented by Mr. G. W. Wood. It was hoped that the Lancashire Sea-Fisheries Committee would by this time have erected a fish hatching-establishment alongside of the Biological Station, especially as Captain Dannevig of the Norwegian hatchery at Flödewig reported favourably on this scheme. Unexpected difficulties have, however, arisen; but the reporter seems confident that sooner or later Port Erin will become an important centre for the propagation of young food-fishes. During 1893 eight dredging expeditions were carried out, mostly under the auspices of a Committee of the British Association, and a considerable amount of the Irish Sea was explored. Over a thousand species of marine animals were collected and identified, of which 38 were new records to the British fauna, 224 new to the particular district, and 17 new to science. The Report contains four plates illustrating the arrangements of the station, a chart of the Irish Sea and a considerable quantity of interesting matter. The expenses of the year amount to about £180, in return for which a great deal of good work has been done; but we regret to see that this is not quite covered by the yearly income. Subscriptions may be sent to Mr. I. C. Thompson, 19, Waverley Road, Liverpool, while applications to be allowed to work at the station or for specimens should be addressed to Professor Herdman, University College, Liverpool.

THE City of London Entomological and Natural History Society has published its Report for the year 1893. The President for this year is Mr. J. E. Clark, and the Secretary Mr. C. Nicholson, 202 Evering Road, Upper Clapton, N.E. This Society has for its object the diffusion of the science of Natural History, by means of papers, discussions, exhibitions, and the formation of collections for reference. The meetings take place on the first and third Tuesdays in each month, from 7.30 to 9.30 p.m., at the London Institution, Finsbury Circus. The entrance fee is two shillings, and the annual subscription five shillings. The papers in the Report deal entirely with entomological matters, but that by Dr. Buckell on "Specific Nomenclature: Present, Past, and Future," should interest

all systematists; while Mr. J. E. Robson's discussion of the cause of Melanism in Lepidoptera, which he ascribes to anything that impedes the direct rays of the sun, should be noted by all who deal with the problem of animal colouration.

THE annual meeting of the Quekett Microscopical Club was held on February 16. Mr. E. M. Nelson, the President, gave an address, in the course of which he reviewed the work done in connection with the microscope during the past year. There was, he feared, a serious danger likely to occur to the "microscopy" of the future owing to the neglect of viewing opaque subjects with a stereoscopic binocular. He did not believe any observer, however eminent, who had not previously passed through a special training of viewing such objects in that way, could form correct ideas of the shape of objects solely by alteration of focus. The Club has now over 340 members, and on May 4 will hold an exhibition at Freemasons' Tavern.

THE lecture programme of the Royal Dublin Society, recently issued, includes "Creeping and Flying Reptiles, and their Modern Descendants," by Professor A. C. Haddon; "Our Western Fisheries," by Rev. W. S. Green; "Nature and Myth in India," by Dr. V. Ball; "Bacteria," by Dr. E. J. McWeeney; "Food, what it is and what it does," by Dr. J. M. Purser; "Reproduction and Rejuvenescence," and "On the Borderline between Animal and Vegetable," by Professor M. M. Hartog. Meanwhile, a very successful course of lectures on Geology has lately been given by Professor Grenville A. J. Cole at Belfast. A specially encouraging feature was the readiness of the students to take up practical work. Interest in glacial problems seems to be reviving in Ireland; we notice in the *Irish Naturalist* for January a paper by Professor Sollas on the drift-beds of the coast south of Dublin, while in the February number of the same magazine Miss Thompson gives an account of what is being done to elucidate glacial problems around Belfast.

THE Natural Science Club of Cambridge University signalised its five-hundredth meeting by a conversazione on March 12, at which lectures were delivered by Messrs. A. Russel Wallace, W. M. Conway, and C. V. Boys. The Brighton and Sussex Natural History and Philosophical Society also held a successful *soirée* at the Brighton Pavilion on March 2, this being the first gathering of the kind for nine years. Mr. W. M. Conway lectured on his Karakoram experiences with the aid of a magic lantern. Among a large number of exhibits calculated to attract the general public, we noticed a case of objects discovered in the Lavant Caves in Goodwood Park, and shown by Mr. J. Lewis, of New Shoreham, who favoured us with the following information:—The caves are situate on the southern slope of the Downs. One of the Roman encampments is in the vicinity, showing the district to have been inhabited at an early day; and other evidence to this effect is afforded by the circumstance that there is a levelled plateau above the caves. The caves were accidentally discovered about a year ago, and were explored by Mr. Dawson and Mr. Lewis. They consist of subterranean passages and galleries branching one from the other, and comprise about an acre of flooring. The average height of the passages is 45 feet, and these are carefully arched. The caves appear to have been hewn out by some early race with picks and wedges of deerhorn. There are indications that the object of the excavators was not, as at Cissbury, to find flints; but the caves were probably used as stores or hiding places by the people dwelling in the neighbourhood. The objects found therein were Neolithic flint implements, flint spear heads and scrapers, a portion of red deer-horn used as a pick, a lamp made of chalk, pottery, bronze articles, a Scandinavian ornament, etc., all which indicate that the occupation of these caves must have extended over a considerable period.

THE excited protests of some of our effeminate fellow-citizens will not, of course, have any effect on the foundation of the British Institute of Preventive

Medicine in Chelsea, as mentioned in our last number (p. 233). This action of what we are pleased to think is only a small section of the enlightened British public may, however, be instructively contrasted with that of certain foreign nations on whom your average Britisher would probably look with scorn. Not to mention New York and Calcutta, the Hungarian Government has established a similar institute at Buda-Pesth, while even "the unspeakable Turk" contemplates founding in the chief town of every province what our contemporary *Nature* is pleased to term an "antirabific laboratory." One such institution has for some time been doing excellent work at Constantinople.

THE North Pole is certainly being boomed just at present. Nansen started for it not so long ago, and Mr. F. G. Jackson has not been back two months from his trial trip. Now we learn that the latter gentleman is soon to set off again, while some Americans are going to have a look for Nansen. Mr. A. C. W. Harmsworth, a Fellow of the Royal Geographical Society, has generously offered to pay all the expenses of a fully-equipped expedition led by Mr. Jackson, which will proceed to Franz Josef Land with a view to exploring it in a northerly direction, and advancing as far as possible towards the Pole. The American expedition, the cost of which will be defrayed by the *Chicago Herald* and other journals, will be commanded by Mr. Walter Wellman. The whaler "Ragnvald Jarl" of Aalsund, has been chartered and will be manned by Norwegians. The immediate object of this expedition will be to discover the whereabouts of Nansen, but a return will be made to Europe for the winter if possible, although stores will be taken which will enable the explorers to winter in Spitzbergen. Both these expeditions will leave about May. We also heard a short time ago of an elaborate expedition inaugurated by Mr. R. Stein, of the United States Geological Survey, and patronised by the National Geographical Society of America. The proposal is to establish a depôt at Cape Tennyson, at the northern entrance to Jones's Sound, and then to follow the coast of Ellesmere Land westward, where an advanced depôt will be established to form the base of operations in the following year, when an endeavour will be made to connect with the discoveries of the Greely party on Greely Fjord. In this connection we may mention that Dr. Erich von Drygalski gives an account in the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* (vol. xx., nos. 8 and 9, 1893) of his journey along the West Coast of Greenland in 1892–3. Starting from Christianshaab, the party journeyed by boat and sledge round the fjords and islands to some miles north of Upernik. The object of the expedition, the cost of which seems to have been defrayed by the Society, was to settle certain points concerning the Ice-Age. In the same journal, Dr. Vanhöffen gives an account of the fauna and flora observed at spring-time in the northern part of the island.

THE British Association will hold its sixty-third meeting at Oxford, in the week beginning Wednesday, August 8. This date is not very convenient for University people, but was chosen to suit the president, Lord Salisbury, the attraction of whose presence will doubtless counterbalance the inconvenience of date. The list of presidents of the various sections has been announced as follows:—(*A*) Mathematics and Physics, Professor A. W. Rücker; (*B*) Chemistry, Professor H. B. Dixon; (*C*) Geology, Mr. L. Fletcher; (*D*) Biology, Professor Bayley Balfour; (*E*) Geography, Captain Wharton; (*F*) Economic Science and Statistics, Professor Bastable; (*G*) Mechanical Science, Professor Kennedy; (*H*) Anthropology, Sir W. H. Flower; and (*I*) Physiology, Professor Schäfer. On the Friday night Professor J. Shield Nicholson of Edinburgh will give a lecture, and on Monday Mr. W. H. White is expected to discourse on naval construction.

THE German Zoological Society will meet in the Zoological Institute, Munich, from April 9 to 11, and on Thursday a visit will be paid to the fish-breeding establishment on the Starnberger See. Foreign Zoologists are specially invited.

CORRESPONDENCE.

Professor Milne-Edwards, M. Grandidier, and Æpyornis.

I venture to think a protest ought to be raised by some British naturalist against the tone of the note in this month's Natural Science on the recent French discovery of *Æpyornis* bones. For many years there has been a generous rivalry, fruitful of enterprise and hard work, between the French and ourselves in the matter of Mascarene Zoological discovery. When, then, as in the case referred to in your article, the French have succeeded in first making known to the world a set of new and interesting fossils, it ill becomes a British Journal of the standing of Natural Science to complain in so cavalier a manner of the exact method in which the discovery is announced.

Were the complaints correct in themselves there would still be little excuse for such an attack, but as a matter of fact the only alleged fault of the authors which, if true, would have caused difficulty to the real worker on specimens, as opposed to the mere critic, practically does not exist at all in the paper referred to. Far from its being the case that "names are applied to collections of limb-bones which may or may not belong to the same species, and no particular bone is taken as the type specimen," the species are distinguished mainly on size, and in four cases out of seven one bone only, either tibia or tarso-metatarsus, has its dimensions fully and exactly recorded, and is therefore clearly in each case the type of the species. Of the other three cases, one was founded on these two bones together and the other two on the two chief leg bones, which may have been found in conjunction, and are in all cases most carefully measured. In a group where size is of the most vital and diagnostic importance, to dismiss these measurements with the remark that "though some measurements are published, we have sought in vain for any adequate diagnosis" is surely rather disingenuous.

Considering the fact that one of the authors attacked, Professor A. Milne-Edwards, holds the highest position in the French zoological world, is a naturalist of whom any nation might be proud, and one to whom many British zoologists, especially those of our National Museum, have been again and again indebted for assistance in various ways, while the other, Mons. Grandidier, simply as a private person, has spent a fortune and a lifetime in furtherance of the scientific knowledge of Madagascar, it would surely have been better had Natural Science recognised in a somewhat different way the magnificent work done by the two eminent Frenchmen it now, not for the first time, so gratuitously attacks.

Natural History Museum, O. T.

March 5, 1894.

[Mr. Thomas seems to believe that some personal animus has influenced our remarks on the work done by MM. Milne-Edwards and Grandidier on the Æpyornithidæ. Let us at once assure him that none more than ourselves have greater respect for the personality of these eminent Frenchmen. It is the principles, not the men, that we attack; and in reply to the second paragraph of Mr. Thomas's letter we have to say:—That in three cases (*Æ. mulleri*, *ingens*, and *betsilei*) specific names are given to a collection of bones, or more than one bone, and since it is not definitely stated that they belong to the same skeleton, it must not be assumed that they do. The inconvenience resulting from this mode of procedure is apparent; for in the case of *Æ. ingens* measurements are given of the femur, tibio-tarsus, and

metatarsus. Now it is stated that the femur is different from that of *Æ. titan* described by Andrews. On referring to the paper quoted, we find that the femur of *Æ. titan* is *not* described, though a femur is provisionally referred to that species. On the other hand, the femur referred to *Æ. ingens* by Milne-Edwards may well have belonged to *Æ. titan;* and, indeed, if the bones referred to *ingens were* associated, then, as far as one can judge from measurements, *ingens* is a synonym of *titan;* for the measurements of the tibia of *ingens* given by Milne-Edwards agree almost exactly with those of the type tibia of *titan*:—

	ingens.	*titan.*
Length	81 cm.	80 cm.
Circumference at narrowest	20·5 ,,	20·7 ,,

It is true that the authors state that *Æ. titan*, though as tall as *Æ. ingens*, was less massive and its leg articulations were smaller. If this opinion is founded on the femur, we have shown that it rests on an insecure foundation. If it rests on the tibia, why are no measurements given of the articular ends in which the main difference seems to lie? Slight variations of measurement are of small value and insufficient for the establishment of specific distinctions, for among individuals in the Ratite birds size varies very greatly.

Of the new genus proposed, "*Mullerornis*," no diagnosis is published, the only detail furnished being "that they are of medium size, not having the massive and heavy appearance of *Æpyornis*, approaching rather the Cassowaries. We only know the bones of the foot."—ED.]

LIVERPOOL MARINE BIOLOGICAL STATION.

IN reply to an enquiry of ours about the Port Erin Biological Station, which we notice on p. 316, Professor Herdman writes to us:—"We are organising a Liverpool Marine Biology Committee dredging expedition for Easter week-end, like those we have had at the same time in recent years. Besides that party, several zoologists and botanists will be at work at the station during the last week of this month and during April, including Professor Weiss, Dr. Hurst, and Mr. Gamble of Owens College, Mr. Beaumont of Emmanuel College, Cambridge, Mr. Thompson of Liverpool, and myself."

A CORRESPONDENT calls our attention to the fact that Professor Lapworth is also "new blood" on the council of the Geological Society. We regret that in our note last month his name was omitted; apparently we could not realise the fact that so distinguished a geologist had not been on the council before.

STRANGE are the ways of the American place-hunter and strange, as we have noted before now, is the system under which scientific appointments are made in the United States. One of our Transatlantic correspondents complains that he has no time for scientific work. "At present," he writes, "I am very busy, being engaged in politics, as I am a candidate before the Republican Convention for the nomination of State Geologist and have the most flattering prospects; my only opponent is a local collector." As our friend might possibly obtain the appointment, we have sufficient regard for his reputation to suppress his name.

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